

FOR AND AGAINST EXPERIMENTS ON ANIMALS

STEPHEN PAGET

WITH AN INTRODUCTION BY
THE RIGHT HON. THE EARL OF CROMER

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LOUIS PASTEUR.

[Frontispiece.]

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FOR AND AGAINST EXPERIMENTS ON ANIMALS

*EVIDENCE BEFORE THE ROYAL
COMMISSION ON VIVISECTION*

[compiled]

BY

STEPHEN PAGET, F.R.C.S.

Hon. Secretary Research Defence Society

WITH AN INTRODUCTION BY

THE RIGHT HON. THE EARL OF CROMER
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PREFACE

THIS book was written at the request of the Committee of the Research Defence Society. All of it, except the Introduction by Lord Cromer, and the last chapter, was seen through the press long before the Royal Commission on Vivisection published its final Report.

It is hoped that the book gives a clear account of the chief results obtained, during the past thirty years, by the help of experiments on animals. So far as possible, this account has been stated in the very words of the men who did the work. They described, from actual experience, decisive events in the Thirty Years' War against the diseases of man and of animals. It is certain that the history of that war is of unfailing interest.

STEPHEN PAGET,

Hon. Secretary Research Defence Society.

21, LADBROKE SQUARE,
LONDON, W.

April 1912.

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INTRODUCTION

BY THE

EARL OF CROMER

THIS work, compiled by Mr. Stephen Paget, to whose energy and perseverance all who are interested in scientific research owe a deep debt of gratitude, will, I think, be found useful to that very numerous body of people who, whilst greatly interested in the subject of Vivisection, have neither the time nor the inclination to study the voluminous Report of the Royal Commission, or the bulky minutes of evidence on which that report is based. It is, indeed, most desirable that the class of persons to whom I here allude should be provided with the means of obtaining information on this subject which will enable them, without undue labour and research, to form their own opinions; for it depends mainly on the views held by non-controversialists,

representing the mass of public opinion in this country, whether the cause of scientific research is to stand or fall. For some years past public opinion on the subject of Vivisection has been in a state of suspense. On the one hand, a very general belief prevailed of the value of the experimental method in the furtherance of scientific research, accompanied by a strong disinclination to impose any hindrances on the pursuit of knowledge beyond those rendered necessary by the obvious dictates of humanity. On the other hand, a stern determination existed to prevent wanton and unnecessary cruelty being inflicted on the brute creation—a determination which had its origin not merely in a laudable repugnance to the infliction of pain on the animals themselves, but was also due to a belief in the demoralisation which would inevitably ensue in the persons of those who were the agents in its infliction. Were the accusations of callous indifference to suffering which were frequently levelled against the experimentalists true? Could the heart-rending accounts of torture inflicted on animals, which were repeated with almost tedious iteration on the one side, and strenuously denied on the other, be substantiated

or not? Were the safeguards already provided by the law against wanton cruelty adequate, or did they require amendment? These were the questions on which the public, puzzled and confused by the conflicting utterances of rival controversialists, expected an authoritative statement of opinion from some impartial and independent source.

Before going any further, I wish to make a personal statement on this subject.

My reasons for accepting the post of President of the Research Defence Society some four years ago were twofold.

In the first place, I have an abhorrence of cruelty to animals, and have at times been fortunate enough to help in some small degree in the furtherance of measures tending to obviate or mitigate such cruelty as is now practised. Deeply convinced as I was of the necessity for promoting scientific inquiry, I certainly should not have associated myself with the Research Defence Society had I not, as a preliminary measure, fully satisfied myself both that the main accusations brought against the experimentalists were wholly devoid of foundation, and, further, that the very eminent and humane men with whom I should be associated were animated with

a detestation of anything approaching to wanton cruelty no less profound and sincere than that which I myself entertained.

My other reason for accepting the position of President of the Society was that I felt strongly that the Vivisectionists, and not their opponents, were the true humanitarians; that they were, under circumstances which rendered them peculiarly liable to misrepresentation, fighting a cause in which not only the whole human race, but also the brute creation, were deeply interested; that, on the one hand, their failure to prove their case would result in what Mr. Stephen Coleridge has characteristically termed the "desolating advance of science"¹ being arrested, and that thus we should have to rely on the researches of more favoured scientists in other countries to arrest disease and to stay the hand of death, whilst, on the other hand, their success would connote the decrease of premature mortality and the mitigation of suffering; that it was not merely unjust, but also unwise, that the medical profession should be allowed to stand alone in the defence of a noble cause; and that their efforts to enlighten the public on the true facts of the case might perhaps

¹ *The Outlook*, March 23, 1912.

in some degree be aided by association with those who, like myself, realised the vast importance of the issue at stake, albeit they could bring no special scientific acquirements to bear on the various technical points involved.

The questions which I have propounded above now admit of being confidently answered, not merely on the authority of any individual scientist, however eminent, but on that of a Royal Commission composed of men of unquestionable ability and impartiality, whose opinions have been formed after an exhaustive and prolonged inquiry into every branch of the question. The consciences of lovers of animals—in other words, those of the vast majority of the inhabitants of these islands—may now be at rest. They may support scientific research in the full assurance that in doing so they will benefit themselves, their friends, their relations, and their descendants, whilst at the same time they will not be giving their adhesion to any principles or practices which the dictates of humanity, reasonably interpreted, could condemn. They may profit by the invaluable knowledge acquired in the physiological laboratory without any reluctance due to

doubts as to the morality of the methods by which that knowledge has been obtained. They may feel confident that if in some extremely rare instances a disposition exists to display indifference or callousness to the sufferings of animals, any such proclivity will be checked, not only by the general opinion of the members of a profession whose main object in life it is to obviate or to mitigate pain, but also by the hand of the law, which already provides against the occurrence of abuse, and which, by the light of the experience gained during the last thirty-six years, it is now proposed to strengthen in respect to some minor points. In a word, as I stated in a letter which I addressed to the press immediately after the issue of the Report of the Commission, "broadly speaking, the supporters of vivisection have proved their case." A more careful study of the Report has tended to confirm me in this conclusion. I proceed to give a short summary of my reasons.

The main question of principle which underlies the whole Vivisection controversy is whether experiments on living animals, however conducted, are or are not justifiable.

A small body of Anti-Vivisectionists consider (p. 55) that "Vivisection is morally unjustifiable whether painful or painless." Mrs. Cook, who gave evidence as one of the representatives of the Parliamentary Association for the Abolition of Vivisection, stated that "nothing less than the total prohibition of all experiments on animals would satisfy them." I wish to speak with respect of those who hold these opinions. I have not the smallest doubt of their sincerity. But I will not attempt to refute their arguments, which have indeed, as I think, been sufficiently refuted by Sir J. Fletcher Moulton and others. The matter is one of opinion, and I fear that between those who hold these extreme views and the general body of the community there lies an abyss which it is impossible to bridge over. All that can be said is that we must agree to differ. The conclusion of the Commission on this point of ethics will, I believe, commend itself to public opinion generally. It is stated (p. 57) in the following terms: "After full consideration we are led to the conclusion that experiments upon animals, adequately safeguarded by law, faithfully administered, are morally justifiable, and should not be prohibited by legislation."

The point next in importance is to consider how far recent progress in medical science is due to the knowledge acquired by experiments conducted on living animals. This question is very fully treated on pp. 21-47 of the Report of the Commission. It would extend this Introduction to undue length were I to attempt to summarise the evidence on which the Commissioners formed their opinions. I confine myself, therefore, to stating their conclusions (p. 47).

They are as follows :

“(1) That certain results, claimed from time to time to have been proved by experiments upon living animals and alleged to have been beneficial in preventing or curing disease, have, on further investigation and experience, been found to be fallacious or useless.

“(2) That, notwithstanding such failures, valuable knowledge has been acquired in regard to physiological processes and the causation of disease, and that useful methods for the prevention, cure, and treatment of certain diseases have resulted from experimental investigations upon living animals.

“(3) That, as far as we can judge, it is highly improbable that, without experiments made on animals, mankind would

at the present time have been in possession of such knowledge.

“(4) That, in so far as disease has been successfully prevented or its mortality reduced, suffering has been diminished in man and in lower animals.

“(5) That there is ground for believing that similar methods of investigation if pursued in the future will be attended with similar results.”

It is to be observed that Dr. George Wilson, who was a member of the Commission, and who (p. 139) though “not an Anti-Vivisectionist,” greatly dislikes vivisection, subscribed to these statements with certain reservations (p. 138). Notably, he held that “the useful results which have been claimed, or may still be claimed [from conducting experiments on living animals] have been enormously over-estimated”; and in connection with the first of the conclusions stated above he expressed an opinion that “the fallacies and failures are far more conspicuous than the successful results.”

As to the value of the knowledge obtained and the practical results which have been achieved by its application, laymen, guiltless of any profound scientific accomplishments,

can, indeed, to a limited extent, form their own opinions on the facts and statistics laid before them, but in the main they must rely on authority. Now, there cannot be a shadow of doubt as to the side on which the weight of authority lies. The members of the medical profession who share the views expressed by Dr. Wilson constitute a very small minority. The Commissioners state (p. 47) that "there can be no doubt that the great preponderance of medical and scientific authority is against the opponents of vivisection. This is more markedly so now than was the case before the Royal Commission of 1875." If, moreover, we look not merely to the numerical majority but also to the personalities of the witnesses, it may, I think, be said, without in any way wishing to disparage the weight to be attached to the opinions of Dr. Wilson and those who share his views, that amongst their opponents are to be found the names of the most distinguished men of science in this and in other countries.

I have no doubt that, as stated by the Commissioners (p. 47), experiments have at times been conducted leading to results which, it was at first thought, would prevent or cure diseases, but which "on further investigation

and experience, have been found to be fallacious or useless." But is this argument, taken by itself, condemnatory of the proceedings of the experimentalists? Far from it. It merely shows the necessity of further and more complete investigation. The pursuit of knowledge in every direction is strewn with the records of false scents which have been followed for a time merely to be abandoned when their falsity was at last recognised. As Prof. Huxley very truly remarked :¹ "The attainment of scientific truth has been effected, to a great extent, by the help of scientific errors." If popular accounts are to be believed, it has, indeed, occasionally happened that some man of genius—an Archimedes or a Newton—suddenly, and even accidentally, hit upon some discovery of vast importance. But, apart from the doubts which hang over the authenticity of episodes of this description, it is to be observed that occasions of this sort are, at best, of very rare occurrence. As a rule, science does not advance by sudden leaps and bounds. More usually, one small forward step leads to another, and it is not till after long, patient, and laborious research, after many disappointments, and after the renunciation of

¹ "Essays," vol. i. p. 63.

numerous errors, that some valuable truth is at length revealed. It often takes years before the discoveries of pure pass into the domain of applied science. I may give one illustration drawn from the history of industrial progress. It took fifteen years of patient research in the chemical laboratory before Von Baeyer determined the constitution and synthesis of indigo, and another twenty years elapsed before the knowledge thus obtained could be applied, with commercial success, to the extraction of indigo from coal tar.

Even assuming, therefore—as I am far from doing—that the somewhat extreme view advocated by Dr. Wilson is correct, and that, in the case of vivisection, “fallacies and failures are far more conspicuous than successful results,” I do not find in this plea any adequate reason for abandoning experiments on living animals. Rather should such considerations serve as an additional stimulant to renewed exertion in the sense of discarding past errors and advancing along the lines which offer the most hopeful prospects of obtaining valuable results.

The argument against the adoption of the experimental method based on past and partial failure appears, therefore, to me to be of little

or no importance. Amongst the many fallacious weapons in the Anti-Vivisectionist armoury, none, I venture to think, is less cogent than the taunt occasionally levelled at their opponents that, in spite of the very numerous experiments so far conducted, no cure for cancer has yet been found. A more reasonable view would appear to be that researches, conducted with a due regard to the dictates of humanity, should be continued in the hope of eventually finding some alleviation for the pain and devastation caused by this terrible scourge.

The Commissioners appear, broadly speaking, to have accepted the validity of the arguments which I have stated above, for, in spite of their acknowledgment of partial failure in the past, they recognise the "valuable knowledge which has been acquired in regard to physiological processes and the causation of disease" by experimental investigations upon living animals; they think it "highly improbable" that without such experiments this knowledge would have been obtained; they hold that, as a result of these experiments, "mortality has been reduced, and suffering diminished in man and in lower animals"; and they conclude by saying that

“there is ground for believing that similar methods of investigation if pursued in the future will be attended with similar results.”

Enthusiasts in the cause of scientific research may perhaps consider that, in expressing these guarded opinions, the Commissioners have done somewhat less than justice to the remarkable achievements of the experimentalists.¹ There is, however, no sort of real cause for discouragement. Far from it. The Commissioners concede the main points for which the Research Defence Society and their sympathisers have persistently pleaded. Moreover, the value of their verdict is, in my opinion, enhanced by the studied moderation of their language, and by the practical unanimity of their decisions. The plea that the knowledge obtained by vivisection is dearly purchased by a sacrifice of animal life may still, to a

¹ It is to be observed that the Commissioners necessarily based their Report on the evidence laid before them, and that, at the time when they commenced taking evidence, the remarkable successes recently achieved by the American scientists at Panama had not been fully disclosed. Those who wish for information as regards the saving of life in tropical climates which has resulted from the adoption of the experimental method cannot do better than study Sir Rubert Boyce's very interesting work entitled “Mosquito or Man?” It was written in 1909, and since then further progress has been made.

certain, though, I trust, to a very limited, extent, hold the field. But we should hear no more of the plea of inutility. The argument that the researches of the Vivisectionists have been barren of result ought to be finally discarded by all save those who are not open to conviction. It has been totally rejected by a large majority of the Commissioners, and is not even really accepted by Dr. Wilson, who does not maintain that the results are of no account, but merely that they have been "enormously over-estimated."

The next point to be considered is the amount of pain inflicted on animals by the adoption of the experimental method.

Let me here call to mind the attitude adopted by the Research Defence Society on this important point. I do not think that any member of the Society has ever maintained—certainly I have never maintained—that in no case was pain inflicted. Our view has been that the vast majority of inoculations involve no pain, or none that may not truly be called trivial; that all experiments involving operation are conducted under anæsthetics; that the anæsthesia is

effective;¹ that the law already provides safeguards against the unnecessary infliction of pain; that if any additional safeguards could be suggested which would not be unduly or vexatiously restrictive, they should be sympathetically considered; and finally, that in those rare instances where some pain was inflicted, the results achieved justified its infliction.

It must, I think, be a great satisfaction to the general body of the community, as it certainly is to myself, to know, on the authority of the Royal Commission, that the charges of cruelty so recklessly levelled against the Vivisectionists have been wholly disproved. In fact, it may be said without exaggeration that, on this vital point, the case of the Anti-Vivisectionists, when submitted to the test of cross-examination, broke down hopelessly. This does not in any way surprise me, for, as I have already mentioned, had I not been aware of the flimsy character of the evidence on which these accusations

¹ It is particularly to be noted that on this subject the Commissioners say (p. 51): "After careful consideration of the whole question of anæsthetics as applied to experimental investigations on living animals, we are led to the conclusion that by the use of one or other or of a combination of several well-known anæsthetics complete insensibility to pain can be secured."

were based, I should never have accepted the Presidency of the Research Defence Society.

The principal protagonist of the Anti-Vivisection cause has been Mr. Stephen Coleridge. He has, as the Commissioners state (p. 14), been "an acute and indefatigable critic" who has devoted "ten years of investigation" to what he considers the abuses which arise under the existing system. He has had at his disposal large sums of money, contributed, it may be assumed, with the express object of enabling him to justify his criticisms. As a result of his investigations, Mr. Coleridge brought twelve specific charges against the Home Office administration of the Act of 1876 (*vide* pp. 11-14 of Report). I need not give the detail of these charges. Not one of them appears to have been substantiated. The Commissioners (p. 14), whilst indicating certain points in respect to which they think that the administration of the Home Office may be open to criticism, add that they "are of opinion that, on the whole, the working of the Act has been performed with a desire faithfully to carry out the objects which its framers had in view."

Mr. Coleridge also brought a charge of callousness against the Home Office on the ground that Prof. Schäfer had been authorised to "drown, resuscitate, and drown again" unanæsthetised dogs. On this point the Commissioners say (p. 16): "Only two unanæsthetised dogs were, however, used, and these were drowned without resuscitation, and, so far as we can judge, suffered no more pain than stray dogs that are destroyed by drowning."

The main charges of cruelty were not, however, advanced by Mr. Stephen Coleridge, but by other witnesses, notably Miss Lindaf-Hageby, Mrs. Cook, Lieutenant-Colonel Lawrie, and Mr. Graham (pp. 16-19). These witnesses were not more successful than Mr. Stephen Coleridge in proving their cases. The Commissioners say (p. 20): "After careful consideration of the above cases we have come to the conclusion that the witnesses have either misapprehended or inaccurately described the facts of the experiments. . . . So far as we can judge we believe that holders of licences and certificates, with rare exceptions, have endeavoured with loyalty and good faith to conform to the provisions of the law."

Finally, in summing up the tactics adopted by the Anti-Vivisectionists, the Commissioners say: ‘ We desire further to state that the harrowing descriptions and illustrations of operations inflicted on animals, which are freely circulated by post, advertisement, or otherwise, are in many cases calculated to mislead the public, so far as they suggest that the animals in question were not under an anæsthetic. To represent that animals subjected to experiments in this country are wantonly tortured would, in our opinion, be absolutely false.’ To those who are familiar with the procedure adopted by some of the Anti-Vivisectionists, this carefully worded verdict would certainly not appear to err on the side of excessive severity. I trust, however, it will do something to guard the public against accepting the misrepresentations of the truth which are forced upon them.

I now come to the question of the recommendations of the Commission.

It is to be observed that all their recommendations are in the sense of further restrictions. None contemplate any relaxation of the existing law in the interests of science. The Commission did, indeed, consider the

question of whether experiments made with a view to acquiring manual dexterity, which the law now forbids, should be allowed (*vide* pp. 59-61), but declined to recommend any alterations in the existing law.

The restrictive recommendations are (p. 65) summarised as follows :

“(1) An increase in the Inspectorate.

“(2) Further limitations as regards the use of curare.

“(3) Stricter provisions as to the definition and practice of pithing.¹

“(4) Additional restrictions regulating the painless destruction of animals which show signs of suffering after experiment.

“(5) A change in the method of selecting and in the constitution of the Advisory Body to the Secretary of State.

“(6) Special records by experimenters in certain cases.”

So far as I can judge, these proposals appear to be reasonable. I cannot think that by their adoption the cause of scientific investigation would be seriously hampered. But much depends on the manner of their execution. I consider it, therefore, essential

¹ “Pithing” is the term applied to operations on the brain and spinal cord, which, without destroying life, are held to deaden all sensation (*vide* p. 53 of Report).

that before any legislation is undertaken or new regulations framed, the advocates of vivisection should be taken into counsel and should have every opportunity afforded to them for expressing their opinions.

Finally, I wish to say a few words on the special question of experiments on dogs, as to which a large section of the public are deeply interested. This subject is treated on pp. 62-63 of the Report. The Commissioners were divided in opinion, but the majority held that "the special enactments now applicable to horses, asses, and mules might be extended to dogs, and also to cats and anthropoid apes."

It is, of course, "logically untenable" (p. 62) to exclude any particular class of animals from being the subject of experiment. On the other hand, almost from the dawn of history¹ dogs have always been regarded as the special friends of man. Nowhere probably does this feeling prevail more strongly than in this country. Without doubt, it is a feeling based on sentiment, but neither scientists nor any other class

¹ εἰς καὶ κυνῶν ἐρινύες—"There is vengeance in heaven for an injured dog." Gilbert Murray, "Rise of the Greek Epic," p. 87.

of persons can afford to neglect sentiment. I should be sorry to see dogs altogether excluded from experiment. I cannot doubt that, by their exclusion, the cause of science would suffer. Prof. Starling, an eminently humane man, said that if dogs were excluded "it would stop all the more advanced observations on digestion, experiments as to the nature of diabetes, and other important cases."¹ Further, the experiments so far conducted on dogs have produced excellent results in the direction of facilitating the treatment of canine diseases. An instance in point is Dr. Copeman's discovery of a vaccine against distemper. Another instance is that the cause of malignant jaundice in dogs has been ascertained, and that the disease can now be treated with far greater prospects of success than heretofore. Moreover, were all experiments on dogs prohibited, the only result, in so far as the dogs themselves are concerned, would be that a few of them, instead of being put to a painless death in the physiological laboratory, would enter a lethal chamber at Battersea or elsewhere. But I think the proposal made by the majority of the Commission to the effect that the legal

¹ See Mr. Paget's Summary of the Evidence, p. 47.

position of dogs should be assimilated to that of horses and mules might reasonably be adopted. At present, under Section 5 of the Act of 1876, no special certificate is required in the case of a dog unless it be proposed either to dispense with the use of anæsthetics, or to keep the animal alive for observation after the effect of the anæsthetic has passed off; whereas in the case of a horse, ass, or mule, a special certificate is invariably required, whether anæsthetics be used or not. If the recommendation of the Commission be adopted, this distinction would disappear. A certificate would always be required before conducting any experiment on a dog.

Such, briefly, are the conclusions at which I have arrived after a careful perusal of the Report of the Royal Commission.

Ill-health and advancing years oblige me to resign the Presidency of the Research Defence Society, but I cannot abandon a work in which I have taken the deepest interest, and in which I have been fortunate enough to be associated with men of profound learning and lofty aspirations, without making a final appeal to the main body of my countrymen and countrywomen. I beg them not to

be scared by the ugly sound of the word Vivisection. I ask them, before being led away by a very natural but, I firmly believe in this case, a misplaced sentiment, to study the facts for themselves by the light of the information now placed at their disposal. Should they comply with this request, I am confident that any preconceived prejudice against the proceedings of the Vivisectionists will be removed. They will find that the appeal to their love of animals has often been made on erroneous statements of fact, and they will come to the conclusion that, under proper safeguards—which none less than myself would wish to see abolished—not only may the practice of vivisection be allowed to continue, but that its arrest would be disastrous to the further progress of humanitarian science in this country.

CROMER.

36, WIMPOLE STREET, W.,
March 27, 1912.



FOR AND AGAINST EXPERIMENTS ON ANIMALS

I

THE ACT OF 1876

THE present Act was founded on the recommendations of a Royal Commission, appointed in June 1875 "to inquire into the practice of subjecting live animals to experiments for scientific purposes, and to consider and report what measures, if any, it may be desirable to take in respect of any such practice." The Commissioners were Lord Cardwell (Chairman), Lord Winmarleigh, Mr. W. E. Forster, Sir John Karslake, Professor Huxley, Sir John Erichsen, and Mr. R. H. Hutton; with Mr. Nathaniel Baker (Secretary). Between July and December 1875, fifty-three witnesses were examined, and 6,551 questions were put and answered; and the Report of the Commission bears date January 8, 1876. Thus, the whole work of the Commissioners was done within seven months.

AN ACT TO AMEND THE LAW RELATING TO CRUELTY TO ANIMALS

Vict. 39 & 40, c. 77

15th August, 1876

Whereas it is expedient to amend the law relating to cruelty to animals by extending it to the cases of animals

which for medical, physiological, or other scientific purposes are subject when alive to experiments calculated to inflict pain :

Be it enacted by the Queen's most Excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons, in this present Parliament assembled, and by the authority of the same, as follows :

1. This Act may be cited for all purposes as "The Cruelty to Animals Act, 1876."

2. A person shall not perform on a living animal any experiment calculated to give pain, except subject to the restrictions imposed by this Act. Any person performing or taking part in performing any experiment calculated to give pain, in contravention of this Act, shall be guilty of an offence against this Act, and shall, if it be the first offence, be liable to a penalty not exceeding fifty pounds, and if it be the second or any subsequent offence, be liable, at the discretion of the court by which he is tried, to a penalty not exceeding one hundred pounds or to imprisonment for a period not exceeding three months.

3. The following restrictions are imposed by this Act with respect to the performance on any living animal of an experiment calculated to give pain ; that is to say,

- (1) The experiment must be performed with a view to the advancement by new discovery of physiological knowledge or of knowledge which will be useful for saving or prolonging life or alleviating suffering ; and
- (2) The experiment must be performed by a person holding such license from one of Her Majesty's Principal Secretaries of State, in this Act referred to as the Secretary of State, as is in this Act mentioned, and in the case of a person holding such conditional license as is hereinafter mentioned,

or of experiments performed for the purpose of instruction in a registered place; and

- (3) The animal must during the whole of the experiment be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain; and
- (4) The animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered; and
- (5) The experiment shall not be performed as an illustration of lectures in medical schools, hospitals, colleges, or elsewhere; and
- (6) The experiment shall not be performed for the purpose of attaining manual skill.

Provided as follows; that is to say,

- (1) Experiments may be performed under the foregoing provisions as to the use of anæsthetics by a person giving illustrations of lectures in medical schools, hospitals, or colleges, or elsewhere, on such certificate being given as in this Act mentioned, that the proposed experiments are absolutely necessary for the due instruction of the persons to whom such lectures are given with a view to their acquiring physiological knowledge or knowledge which will be useful to them for saving or prolonging life or alleviating suffering; and
- (2) Experiments may be performed without anæsthetics on such certificate being given as in this Act mentioned that insensibility cannot be produced without necessarily frustrating the object of such experiments; and

(3) Experiments may be performed without the person who performed such experiments being under an obligation to cause the animal on which any such experiment is performed to be killed before it recovers from the influence of the anæsthetic on such certificate being given as in this Act mentioned, that the so killing the animal would necessarily frustrate the object of the experiment, and provided that the animal be killed as soon as such object has been attained ; and

(4) Experiments may be performed not directly for the advancement by new discovery of physiological knowledge, or of knowledge which will be useful for saving or prolonging life or alleviating suffering, but for the purpose of testing a particular former discovery alleged to have been made for the advancement of such knowledge as last aforesaid on such certificate being given as in this Act mentioned that such testing is absolutely necessary for the effectual advancement of such knowledge.

4. The substance known as urari or curare shall not for the purposes of this Act be deemed to be an anæsthetic.

5. Notwithstanding anything in this Act contained, an experiment calculated to give pain shall not be performed without anæsthetics on a dog or cat, except on such certificate being given as in this Act mentioned, stating, in addition to the statements herein-before required to be made in such certificate, that for reasons specified in the certificate the object of the experiment will be necessarily frustrated unless it is performed on an animal similar in constitution and habits to a cat or dog, and no other animal is available for such experiment ; and an experiment calculated to give pain shall not be performed on any horse, ass, or mule except on such certificate being given as in this

Act mentioned that the object of the experiment will be necessarily frustrated unless it is performed on a horse, ass, or mule, and that no other animal is available for such experiment.

6. Any exhibition to the general public, whether admitted on payment of money or gratuitously, of experiments on living animals calculated to give pain shall be illegal.

Any person performing or aiding in performing such experiments shall be deemed to be guilty of an offence against this Act, and shall, if it be the first offence, be liable to a penalty not exceeding fifty pounds, and if it be the second or any subsequent offence, be liable, at the discretion of the court by which he is tried, to a penalty not exceeding one hundred pounds or to imprisonment for a period not exceeding three months.

And any person publishing any notice of any such intended exhibition by advertisement in a newspaper, placard, or otherwise shall be liable to a penalty not exceeding one pound.

A person punished for an offence under this section shall not for the same offence be punishable under any other section of this Act.

Administration of Law

7. The Secretary of State may insert, as a condition of granting any license, a provision in such license that the place in which any experiment is to be performed by the licensee is to be registered in such manner as the Secretary of State may from time to time by any general or special order direct; provided that every place for the performance of experiments for the purpose of instruction under this Act shall be approved by the Secretary of State, and shall be registered in such manner as he may from time to time by any general or special order direct.

8. The Secretary of State may license any person whom he may think qualified to hold a license to perform experiments under this Act. A license granted by him may be for such time as he may think fit, and may be revoked by him on his being satisfied that such license ought to be revoked. There may be annexed to such license any conditions which the Secretary of State may think expedient for the purpose of better carrying into effect the objects of this Act, but not inconsistent with the provisions thereof.

9. The Secretary of State may direct any person performing experiments under this Act from time to time to make such reports to him of the result of such experiments, in such form and with such details as he may require.

10. The Secretary of State shall cause all registered places to be from time to time visited by inspectors for the purpose of securing a compliance with the provisions of this Act, and the Secretary of State may, with the assent of the Treasury as to number, appoint any special inspectors, or may from time to time assign the duties of any such inspectors to such officers in the employment of the Government, who may be willing to accept the same, as he may think fit, either permanently or temporarily.

11. Any application for a license under this Act and a certificate given as in this Act mentioned must be signed by one or more of the following persons ; that is to say,

The President of the Royal Society ;

The President of the Royal Society of Edinburgh ;

The President of Royal Irish Academy ;

The Presidents of the Royal Colleges of Surgeons in
London, Edinburgh, or Dublin ;

The Presidents of the Royal Colleges of Physicians in
London, Edinburgh, or Dublin ;

The President of the General Medical Council ;

The President of the Faculty of Physicians and Surgeons of Glasgow;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science;

and also (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

12. The powers conferred by this Act of granting a license or giving a certificate for the performance of

experiments on living animals may be exercised by an order in writing under the hand of any judge of the High Court of Justice in England, of the High Court of Session in Scotland, or of any of the superior courts in Ireland, including any court to which the jurisdiction of such last-mentioned courts may be transferred, in a case where such judge is satisfied that it is essential for the purposes of justice in a criminal case to make any such experiment.

Legal Proceedings

13. A justice of the peace, on information on oath that there is reasonable ground to believe that experiments in contravention of this Act are being performed by an unlicensed person in any place not registered under this Act may issue his warrant authorising any officer or constable of police to enter and search such place, and to take the names and addresses of the persons found therein.

Any person who refuses admission on demand to a police officer or constable so authorised, or obstructs such officer or constable in the execution of his duty under this section, or who refuses on demand to disclose his name or address, or gives a false name or address, shall be liable to a penalty not exceeding five pounds.

14. In England, offences against this Act may be prosecuted and penalties under this Act recovered before a court of summary jurisdiction in manner directed by the Summary Jurisdiction Act.

In England "Summary Jurisdiction Act" means the Act of the session of the eleventh and twelfth years of the reign of Her present Majesty, chapter forty-three, intituled "An Act to facilitate the performance of the duties of justices of the peace out of sessions within England and Wales with respect to summary con-

victions and orders," and any Act amending the same.

"Court of summary jurisdiction" means and includes any justice or justices of the peace, metropolitan police magistrate, stipendiary or other magistrate, or officer, by whatever name called, exercising jurisdiction in pursuance of the Summary Jurisdiction Act: Provided that the court when hearing and determining an information under this Act shall be constituted either of two or more justices of the peace in petty sessions, sitting at a place appointed for holding petty sessions, or of some magistrate or officer sitting alone or with others at some court or other place appointed for the administration of justice, and for the time being empowered by law to do alone any act authorised to be done by more than one justice of the peace.

15. In England, where a person is accused before a court of summary jurisdiction of any offence against this Act in respect of which a penalty of more than five pounds can be imposed, the accused may, on appearing before the court of summary jurisdiction, declare that he objects to being tried for such offence by a court of summary jurisdiction, and thereupon the court of summary jurisdiction may deal with the case in all respects as if the accused were charged with an indictable offence and not an offence punishable on summary conviction, and the offence may be prosecuted on indictment accordingly.

16. In England, if any party thinks himself aggrieved by any conviction made by a court of summary jurisdiction on determining any information under this Act, the party so aggrieved may appeal therefrom, subject to the conditions and regulations following :

- (1) The appeal shall be made to the next court of general or quarter sessions for the county or place in which

the court of appeal has arisen, holden not less than twenty-one days after the decision of the court from which the appeal is made; and

- (2) The appellant shall, within ten days after the cause of appeal has arisen, give notice to the other party and to the court of summary jurisdiction of his intention to appeal, and of the ground thereof; and
- (3) The appellant shall, within three days after such notice, enter into a recognizance before a justice of the peace, with two sufficient sureties, conditioned personally to try such appeal, and to abide the judgment of the court thereon, and to pay such costs as may be awarded by the court, or give such other security by deposit of money or otherwise as the justice may allow; and
- (4) Where the appellant is in custody the justice may, if he think fit, on the appellant entering into such recognizance or giving such other security as aforesaid, release him from custody; and
- (5) The court of appeal may adjourn the appeal, and upon the hearing thereof they may confirm, reverse, or modify the decision of the court of summary jurisdiction, or remit the matter to the court of summary jurisdiction with the opinion of the court of appeal thereon, or make such other order in the matter as the court thinks just, and if the matter be remitted to the court of summary jurisdiction the said last-mentioned court shall thereupon re-hear and decide the information in accordance with the order of the said court of appeal. The court of appeal may also make such order as to costs to be paid by either party as the court thinks just.

17. In Scotland, offences against this Act may be prosecuted and penalties under this Act recovered under the provisions of the Summary Procedure Act, 1864, or if a person accused of any offence against this Act in respect of which a penalty of more than five pounds can be imposed, on appearing before a court of summary jurisdiction, declare that he objects to being tried for such offence in the court of summary jurisdiction, proceedings may be taken against him on indictment in the Court of Justiciary in Edinburgh or on circuit.

Every person found liable in any penalty or costs shall be liable in default of immediate payment to imprisonment for a term not exceeding three months, or until such penalty or costs are sooner paid.

18. In Ireland, offences against this Act may be prosecuted and penalties under this Act recovered in a summary manner, subject and according to the provisions with respect to the prosecution of offences, the recovery of penalties, and to appeal of the Petty Sessions (Ireland) Act, 1851, and any Act amending the same, and in Dublin of the Acts regulating the powers of justices of the peace or of the police of Dublin metropolis. All penalties recovered under this Act shall be applied in manner directed by the Fines (Ireland) Act, 1871, and any Act amending the same.

19. In Ireland, where a person is accused before a court of summary jurisdiction of any offence against this Act in respect of which a penalty of more than five pounds can be imposed, the accused may, on appearing before the court of summary jurisdiction, declare that he objects to being tried for such offence by a court of summary jurisdiction, and thereupon the court of summary jurisdiction may deal with the case in all respects as if the accused were charged with an indictable offence and not an offence

punishable on summary conviction, and the offence may be prosecuted on indictment accordingly.

20. In the application of this Act to Ireland the term "the Secretary of State" shall be construed to mean the Chief Secretary to the Lord Lieutenant of Ireland for the time being.

21. A prosecution under this Act against a licensed person shall not be instituted except with the assent in writing of the Secretary of State.

22. This act shall not apply to invertebrate animals.

It will be noted that the Act makes no mention of inoculations. Ninety-five per cent. of all experiments on animals, in this country, at the present time, are inoculations, or similar experiments, which none of them involve any sort or kind of cutting operation on any animal. In 1876, the study of germs was a new method. Bacteriology, as we now think of it, is hardly recognised in the text of the Act. That is to say, the Act was drafted without any direct reference to ninety-five out of every hundred experiments now made under it. Therefore, every inoculation, and every similar experiment, has to be scheduled under Certificate A, as an experiment performed without anæsthetics. It must be scheduled somehow, and that is the only certificate possible for the purpose.

Certificate A is never allowed for experiments involving a cutting operation. Nobody desiring to operate on any animal would ever dream of applying for Certificate A for that purpose.

In all experiments under licence alone, without certificates, the animal must be under an anæsthetic throughout the whole of the experiment; and, if the pain is likely to continue after the effect of the anæsthetic has ceased, or

if any serious injury has been inflicted on the animal, it must be killed before the anæsthesia has passed off.

Certificate B dispenses with the obligation to kill the animal before the anæsthesia has passed off. It is used for those experiments where an operation is done under anæsthesia, and the animal is allowed to come round after the operation, and to be kept for observation. The operation must be done by the antiseptic method, as it would be done on a human patient. If the wound does not heal well, and matter is formed, the animal must be killed under an anæsthetic. Once the operation has been done, no further experiment or stimulation or observation of any painful nature is allowed to be made on the animal, unless it is again put under an anæsthetic.

Certificate C permits experiments, under anæsthesia, by way of illustration of lectures; provided, "that the proposed experiments are absolutely necessary for the due instruction of the persons to whom such lectures are to be given, with a view to their acquiring physiological knowledge, or knowledge which will be useful to them for saving or prolonging life or alleviating suffering."

Certificate D has fallen into disuse. It permits experiments for the purpose of testing former discoveries alleged to have been made; provided that such testing is "absolutely necessary for the effectual advancement of such knowledge."

Certificate E is linked with Certificate A, in those cases where dogs and cats are to be used for experiments under Certificate A; provided, "that the object of any such experiment will be necessarily frustrated unless it is performed on an animal similar in constitution and habits to a dog or cat, and that no other animal is available for any such experiment."

Certificate EE is linked with Certificate B, as Certificate E is linked with Certificate A.

Certificate F permits experiments on horses, asses, or mules, under the conditions of the licence. If Certificate A or Certificate B be also necessary, Certificate F may be linked with either of them ; provided, "that the object of any such experiment will be necessarily frustrated unless it is performed on a horse, ass, or mule, and that no other animal is available for such experiment."

The wording of these certificates has been modified, now and again, by the Home Office ; and, of course, the Home Office may impose special conditions on the allowing of this or that certificate to an applicant. The forms, which have been quoted in part only, are printed in full in Appendix I, Vol. V, Minutes of Evidence, Royal Commission on Vivisection, March 1908. As examples of the administration of the Act, we may take the following instances. To operate on a cat, and keep it for observation after the operation, a licence, Certificate B, and Certificate EE are necessary. To inoculate a dog for the study of distemper, a licence, Certificate A, and Certificate E are necessary. To demonstrate to a class, on a rabbit, some physiological fact—the rabbit being under an anæsthetic and killed, under the anæsthetic, after the demonstration—a licence and Certificate C are necessary.

Though it is true that some experiments under Certificate A involve pain, yet it seems hardly reasonable that inoculations should be represented to the public as "vivisection." For example, in 1908, no less than 12,500 observations were made for the Royal Commission on the Disposal of Sewage. Young fishes and fishes' eggs were exposed to the influence of effluents in different stages of purification and dilution. That is all that was done to them. Under the Act, every one of these 12,500 observations had to be returned to the Home Office as "an experiment performed on a living animal without anæsthetics."

The present Royal Commission on Vivisection was appointed in 1906, "To inquire into and report upon the practice of subjecting live animals to experiments, whether by vivisection or otherwise, and also to inquire into the law relating to that practice and its administration; and to report whether any, and, if so, what changes are desirable." The Commissioners were Lord Selby (Chairman), Colonel Lockwood, Sir William Church, Sir William Collins, Sir John McFadyean, Sir Mackenzie Chalmers, Mr. Ram, Dr. Gaskell, Mr. Tomkinson, and Dr. George Wilson, with Captain Bigham (Secretary). The Commission has lost by death two of its members, Lord Selby and Mr. Tomkinson. On the death of Lord Selby, Mr. Ram became Chairman. The first sitting to hear evidence was on October 31, 1906; the last was on March 25, 1908.¹ Fifty-eight witnesses were examined, some of them at very great length—Professor Starling for two days and a half, Mr. Coleridge for three days—and 21,761 questions were put and answered. The evidence was published from time to time, without waiting till it had all been given. It is contained in five Blue books, to be purchased from Wyman & Sons, Fetter Lane, E.C., or through any bookseller.

The witnesses first examined were the officials representing the Home Office: Mr. Byrne, Mr. Thane, Sir James Russell, and Sir William Thornley Stoker.

MR. W. P. BYRNE, C.B., *October 31, 1906*

Mr. Byrne, a Principal Clerk in the Home Office, described the events which led to the 1875 Commission,

¹ This last sitting was held for the purpose of inquiring into certain statements made by Colonel Lawrie (see Chapter X). Except for this special occasion, the Commission ceased to hear evidence on December 18, 1907.

and gave a summary of the Act. He stated that no fresh licence or certificate was granted or allowed to come into operation without the authority of the Under Secretary. Every proposed investigation of a novel, important, or painful character was submitted by the Under Secretary to the Home Secretary personally—"Very rarely with reference to its painfulness, because the operations in which considerable pain seem probable are very few indeed; but if it is a novel investigation—one that might attract popular interest or attention, or one which is of an important character in itself, or which is, for instance, to aid in carrying out the work of a Royal Commission or anything of that sort, or especially to aid in carrying out the work of the Home Office in connection with factories and dangerous trades—anything of that sort would be laid before the Secretary of State, and occasionally a case would be laid before him on the ground of the probability of considerable pain." If it were a case of simple inoculation, or of lancing a superficial vein, anæsthetics were not required; if it were more severe than that, they were. The condition that an animal, after inoculation, should be killed immediately the main result of the experiment had been attained, if it were found to be in pain which was either considerable in degree or likely to endure, was not in the Act itself, but was devised by the Home Secretary; so was the condition that all animals under Certificate B should be treated with strict antiseptic precautions, and that if these failed, and pain resulted, the animal should be killed immediately; so was the condition that any subsequent operative procedure on such animals must be carried out under anæsthetics of sufficient power to preclude pain.

Every application for a new licence or certificate was referred, by the Home Secretary, to the Association for

the Advancement of Medicine by Research.¹ Over every such application, also, one of the inspectors was consulted.

Mr. Byrne stated that, since 1876, four licences had been revoked as a punishment for misconduct, and, in a considerably larger number of cases, a licence had been suspended. Otherwise, the great bulk of irregularities had been of a trifling character, calling for no step beyond a warning or rebuke. For example, a licensee might forget to apply for the annual renewal of his licence; or, what most frequently happened, he might give an anæsthetic before making an inoculation under Certificate A, which dispenses with anæsthetics.

Asked as to the qualifications of licensees, Mr. Byrne said that in 1888 one licence was granted to a student of medicine, and another was granted to another student in 1890; with special arrangements in each case for supervision of their work. Two or three graduates in arts also, studying medicine, but not yet qualified, had received licences to work under supervision. Persons not qualified by scientific knowledge had been refused:—

“Thus, for instance, laboratory attendants have been refused. A clergyman, who said that he had been for many years a student of anatomy, and was much interested in it, was told that the Secretary of State could not entertain his application. A gentleman of independent means, pursuing the study of bacteriology for the love of

¹ The Council of this Association, which was founded in 1882, offered that year to the Home Office “to render within their province such aid or advice as would tend to facilitate the administration of the statute without trenching upon the absolute discretion committed to the Home Secretary.” Sir William Harcourt, then Home Secretary, at once accepted this offer; and, in December 1882, it was decided that no application under the Act should be entertained unless it had been recommended to him by the Council of the Association.

the thing, asked for a licence, but he was told that it could not be granted to him. Several other applications of a similar sort have been refused; and probably more applications than the Home Office knows of have been prevented, by not being encouraged by the inspector who has first heard of them: but Mr. Thane will be able to tell you about that. A manager of a mine, who wished to test the effect of mine-gases on birds and small animals, was refused a licence for that purpose."

Asked what was done in cases where it was doubtful whether a licence was or was not needed (*e.g.* whether the feeding of kittens on cow's milk instead of cat's milk was an experiment under the Act¹), he said that the Home Secretary, in such cases, would give the licence, to protect the applicant.

Later, the following questions were put and answered:

Q. As a matter of fact, every certificate or licence goes through you, does it not?—*A.* Yes.

Q. Very often it goes to the Assistant Secretary before it comes to the Under Secretary?—*A.* Yes.

Q. So that it goes through three sets of eyes in the Home Office?—*A.* Yes.

Q. And then the Under Secretary determines whether it is of a novel character, or important enough to go on to the Secretary of State?—*A.* Yes.

Q. Then may I take it that every licence which is applied for has to pass through not only three sets of eyes, but four sets of authorities, before it is granted?—*A.* Yes.²

¹ As a matter of fact, Certificate A was applied for, to cover these observations, and was granted by the Home Office, to ensure the protection of the licensee from vexatious prosecution. See Minutes of Evidence, vol. i. p. 10.

² Every application must be recommended by the signature of a President and a Professor. For the list of the few persons who have the right to recommend applications, see the text of the Act.

Q. Then it has to be sent by the Home Office to the Society of Research?—A. Yes.¹

Q. Then it has to go to the Inspector?—A. Yes.

Q. And then, again, it has to run the gauntlet of either two or three officials of the Home Office?—A. Yes.

Q. And the same with a certificate?—A. That is so.

In reply to further questions, Mr. Byrne pointed out that probably the widest form of Certificate A was that for the study of infective diseases, as in work done for Government Departments. The Board of Agriculture and the Local Government Board both had occasion to employ persons to make experiments. The Home Office, indirectly, had been the occasion of a good many experiments, in connection with dangerous trades. The investigations of doctors who had advised them on that subject, Dr. Oliver and other well-known doctors, had necessarily involved experiments on animals; and the Home Office had willingly made use of the results of such investigations.

MR. G. D. THANE, LL.D., *November 7, 14, 21, 1906*

Mr. Thane, Professor of Anatomy in University College, London, and LL.D. of the University of Edinburgh, stated that he had been Inspector for the whole of Great Britain since 1899; Sir James Russell acted under him, as Inspector for Scotland and the Northern Counties; Sir W. Thornley Stoker was Inspector for Ireland. Asked as to his duties he answered, "The Act only mentions that the inspector shall visit from time to time all registered places for the purpose of securing compliance with the provisions of this Act: but, in addition to paying visits of inspection to the places where experiments are desired to be, or are, per-

¹ That is, the Association for the Advancement of Medicine by Research, not the Research Defence Society.

formed, all papers relating to registration of places, applications for licences, certificates granted, and questions whether in Parliament or by private communication, and other matters arising out of the administration of the Act, are referred to me (or in the first place to my colleague of the Northern district), and I advise upon them. I or my colleague read the publications containing accounts of experiments sent in by the licensees, and report as to the due observance of the provisions of the Act, and of the conditions under which the licences were granted, or the certificates allowed to come into operation. I receive reports of experiments from the licensees, and from them I prepare the Return which is presented to Parliament each year. I believe that no important step is taken in the administration of the Act without my being consulted."

Asked about registration of premises, he answered: "When an application for the registration of premises for the performance of experiments comes to me, I ascertain in the first place what is proposed to be done, what kind of experiments, and with what object; and I consider, also, whether there are any circumstances as to ownership, control, or the like, which would make it undesirable to grant registration. These being found satisfactory, I visit the place, and see if the premises are suitable for the proposed experiments, that no danger or inconvenience to the public is likely to arise, and that there is proper accommodation for the animals. When I am satisfied as to all of these, I recommend registration." Mr. Thane said that the only private premises registered for the performance of experiments were the pharmaceutical laboratories of Messrs. Wellcome and Co., and of Messrs. Brady and Martin, and a tract of heather at Frimley. There were six licensees authorised to perform experiments outside registered places, so that they might study on the

spot outbreaks of disease among animals. In three other cases, within the last few years, licensees had been allowed to perform experiments in connection with caisson disease, on the effects following exposure to the influence of compressed air, at places that were not registered. These were the premises of a firm of diving engineers, and the bridge-works at Newcastle-on-Tyne.

Asked what he considered the necessary qualifications for a licence, Mr. Thane answered that the applicant must have had a proper training to do the work, and must have sufficient knowledge to be able to arrange the work and carry it out. He did not inquire about humanity: he made inquiries from persons who would be likely to know what were the qualifications of the applicant: and he made frequent inquiries as to the nature and probable value of the proposed experiments.

Asked about inspection, he answered, "I pay visits of inspection almost always without notice. If I particularly want to meet a licensee, I make an appointment with him, but this happens infrequently. And when I visit a place at a distance, and where but little experimental work is done, I sometimes, but not always, send word that I am coming, as otherwise I may not meet anybody, and possibly may not be able to get into the premises. These several places are visited, on an average, about three times a year. A few, where there is not much experimental work going on, are not visited so frequently, some are visited more frequently, but most of the educational institutions are visited at least once in each of the three terms of the academic year. When making an inspection, I go round the laboratory, generally accompanied by a licensee, and I discuss with the licensees what kind of experiments they are doing. I explain to them what the requirements of the Act, and of the Secretary of State, are: much time is often occupied

in giving information, explanation, and advice to licensees or persons wishing to obtain licences, both personally and by correspondence. I often inspect their registers, and I make a careful examination of the animals in the laboratory or in the animal houses, both those in stock and those which are under experiment: and the condition of the latter, especially, is noted and recorded. I see habitually large numbers of animals that are being experimented on under Certificate A, a few under Certificates A and E (cats and dogs), very few under A and F (horse, ass, or mule): that is, without the use of anæsthetics. The animals are mostly guinea-pigs: there are a good many rabbits, mice, and rats, a few monkeys, cattle, goats, and pigs, and a few birds, fowls and pigeons. The general impression that one gets from seeing these animals is that they have little or nothing the matter with them. It is a constant experience that one cannot tell by inspection whether anything has been done to them or not. The same thing is experienced by the licensees, and even at times by the attendants, who are naturally the most familiar with the animals, unless there is a systematic separation of the experimental from the normal animals: without that separation, every one is dependent on the labels, or perhaps on markings on the animals. In explanation of this, it is to be remarked, firstly, that a large number of these experiments are negative; that is, nothing follows the injection or inoculation: the animal remains in perfect health. Secondly, in infection with tuberculosis, and in testing anti-diphtheritic serum, which together make up a considerable proportion of the experiments in question, the animals do not appear to be in pain, or show any indications of suffering, for at all events a considerable time, if at all; unless a very large dose of diphtheritic toxin has been administered, and that is not common: that means, of course, that it would act

very quickly. . . . On the other hand, it is certain that in some cases of this group the infection or injection is followed by great pain and suffering. I may mention the injection of tetanus toxin and the infection with plague: also the insertion of certain drugs. . . . These form but a very small proportion of the experiments in question. The investigations on the plague are very limited; and I very seldom see animals that have been injected with tetanus toxin."

Asked about experiments under Certificate B, Mr. Thane said: "When, in operations under these certificates, the operation has been performed and the wound has healed, it does not follow that the animal remains in a state of pain or suffering. Indeed, in a large number of cases the health of the animal is not disturbed, as the affection produced is strictly local, and not of a painful character. Thus, in a large class of experiments—those having for their object the elucidation of the structure and functions of the nervous system—such proceedings as section of the brain or spinal cord, excision of a part of the brain, and division of nerves, do not in themselves give rise to pain; their effect is to prevent sensation being transmitted or perceived: so that they cannot produce pain in the affected or paralysed parts. Again, excisions of organs, another considerable class of experiments, are not necessarily followed by suffering. One kidney may be removed, one supra-renal body, or the uterus, or the spleen, without the health of the animal being affected in any way that we can recognise. I have seen many animals in which operations of this kind have been performed, and the animals have seemed none the worse for it." Mr. Thane went on to say that there were more severe cases, under Certificate B, which did cause pain; but the great majority of experiments under this certificate did not cause any considerable pain.

Asked about the work of inspection, he said that he gave a great deal of time to it, but not the whole of his time. "The subject is growing, and I think that additional inspection will soon be required. I have managed to keep abreast of it up to the present, but it is getting to be more than one man can manage." Apart from inoculation experiments, he had seen last year (he was ill during part of the year) fifteen experiments, and twenty-five the year before, and twenty-eight the year before that. Asked about Certificate C, he said that he thought, for the effective teaching of physiology, it was necessary to show experiments.

The following questions were put and answered :

Q. (Sir Mackenzie Chalmers) I do not know who laid down the pretty obvious principle that you are not a detective ; but if any irregularity under the Act came to your notice, would it be part of your functions to communicate it to the Home Office ?—*A.* Certainly, immediately.

Q. On the operation of the Act generally, are you satisfied that, in the main, experiments are carried out as humanely as possible ?—*A.* I think so ; I am quite satisfied that experiments are carried out as humanely as possible.

Q. (Mr. Ram) Do you think that there would be any advantage in having a greater number of inspectors and inspections ?—*A.* I do not think you would get any advantage by it. I do not think that any abuses are going on that you would stop. It would be only a satisfaction to the public perhaps, but that is the only advantage it would be.

Q. Would you keep the amount of inspection that you have ?—*A.* I should of course suggest that the inspector's staff should grow with the increased work demanded of it.

Asked about certain statements in "The Shambles of Science," he said that one had no foundation, another was

absolutely a misrepresentation, and a third made a suggestion that was ridiculous. Asked whether antitoxins and vaccines ought not to be prepared in public laboratories rather than in commercial laboratories, he said that he would prefer them under public control. Asked about the case of the "Brown Dog," he said that it raised rather a fine legal point; he did not think that the Act had been contravened; he did not think, if he had known beforehand, that he would have interfered from the point of view of the animal; but, from the point of view of public feeling, he would have said that they had better not give the demonstration.

He was asked, as follows, about certain experiments made, in dogs, on the influence of micro-organisms on gall-stones placed in the gall-bladder :

Q. Would you say that those dogs must have suffered very severely?—*A.* I am sure they did not. I watched those dogs most carefully. I examined them repeatedly; and they reported on them four times. The wounds healed up perfectly. I have had the dogs out of their cages, and they have pawed over me, and danced around me, and I felt their abdomens, and I am satisfied that they were not suffering.

Q. That was after the operation?—*A.* Yes.

Q. But did you see them when they had stones in the gall-bladders?—*A.* Yes.

Q. Do you mean to say that you do not think they suffered then?—*A.* I am quite sure they did not.

Asked about subdural inoculation of rabbits for the diagnosis of rabies, he said that he was quite satisfied that the rabbits did not suffer pain. Asked about the inoculation of tubercle, he said that he was sure the guinea-pigs did not feel pain in the affected glands. These glands were not inflamed: "and every doctor will confirm me when I

say that tuberculous glands, that young people get, are not in themselves painful if they are not inflamed." Later, the following questions were put and answered :

Q. (*Sir William Collins*) Both Mr. Byrne and yourself told us that the returns, made annually under the Act, for some years purported to show a distinction between painful and painless experiments?—*A.* That title was on the outside of the Report.

Q. They purported to show a distinction between painful and painless experiments?—*A.* Only to that extent. There was no difference made in the interior of the Report, in the body of the Report.

Q. Is it or is it not a fact, that for some years the returns made under the Act purported to show a distinction between painful and painless experiments?—*A.* If that title on the outside of the Report is a purport, it is so ; but that was all. The only change that was made was on the cover of the Report. There is no change whatever made in the manner of the return, the nature of the return.

Q. Mr. Byrne told us, I think, that last year the return did not even profess to make such distinction?—*A.* That is so.

Q. So that prior to last year a distinction was made?—*A.* In the title.

Q. That is to say, it purported to show painless experiments and painful experiments?—*A.* Yes ; but I think we may say that it did not do it any more than the present one does.

Q. I wanted to ask you what value was to be attached to that distinction, which, for many years, was made in the return?—*A.* I always thought none.

SIR JAMES RUSSELL, LL.D., F.R.C.P. EDIN.,

November 7, 1906

Sir James Russell, Fellow of the Royal Society of Edinburgh, and sometime Lord Provost and Lord Lieutenant of Edinburgh, stated that he had been Assistant Inspector since 1890.

Asked his opinion as to the present administration of the Act, he answered, "I think the Vivisection Act has been worked with extreme care, and with a strictness greatly surpassing that used in the administration of any other Act with which I have been concerned." The following questions were put and answered :

Q. (Sir William Collins) I think you had some instructions from the late Dr. Poore, who was at one time Chief Inspector?—*A.* Yes.

Q. What were they?—*A.* When I took office first, he wrote to me to tell me that I was to visit registered places about three times a year ; but he expressly said that I was not expected to act as a detective. In practice, some registered places have been visited much more frequently. Last year I sent in reports of 145 visits.

Q. What is the number of registered places within your purview?—*A.* There are 38.

Q. Have you been instructed to make surprise visits?—*A.* Never.

Q. Do you, as a matter of fact, give notice of your intention to visit?—*A.* Very rarely. It does not suit my own convenience to give notice. In former times, I was really so busy that I could not have held the office if I had had to make appointments. I just go when it suits myself ; and I often do not know when and where I am going. It happens that I find I am free for a whole day or half a day, and I use it for inspection.

Q. Have you encountered any difficulties in making your observations in various laboratories?—*A.* I have never had the least difficulty. I simply walk into them, and always have found the doors open. I usually look first for the licensee, but sometimes I go to the animal house first, and then go there a second time with the licensee. Last week I found no one in a laboratory, and I walked all through the place, and went to the animal house to examine the animals; and then, when I was on the point of coming away, I met an attendant, and went back and visited the animals with him, so as to get information.

Q. Do you visit in the absence of the licensee?—*A.* Yes, often.

Q. Have you had any difficulty in distinguishing between animals which have been experimented upon and those which have not?—*A.* I find constant difficulty in that respect.

Q. Why is that?—*A.* If one sees animals which are not separate—those that are used from the unused—in most cases I defy any one to tell which is which.

Q. Have you ever found any animals, which had been the subject of experiment, suffering pain?—*A.* Yes, I have; at least, what I considered pain.

Q. Do you desire to specify any class of cases which are painful?—*A.* I have spoken to licensees very frequently upon the question of pain after experiments, and they have told me of a few instances.

Q. Can you mention them to the Commission?—*A.* First of all, there were cases following the administration of certain drugs (chromic acid, and coriaria). I saw a great many rats and some guinea-pigs infected with plague, during the last plague outburst in the country, in at least three places; and one or two of those were wild rats, which seemed to me miserable and dying, evidently in

suffering. Then another animal poison, of which I heard as causing pain, is snake-venom. I have not seen any indication of pain in the behaviour of an animal from cobra-venom, but I have been told—I did not see it myself—that daboya-venom put into a cat caused indications of severe pain. Next, as to thyroid glands and parathyroid glands. I happen to have seen a great number of cases of excision of the thyroid gland, from the year 1890 onwards; and most of the animals, the great majority of them, did not seem to suffer at all; but, in a few rather recent cases, when these glands were removed from carnivorous animals, the removal gave rise to nervous symptoms in some of them, which I thought indicated pain. I saw two cases myself.

Q. (Sir John McFadyean) I notice that you indicated that, in your opinion, only a minority of animals which have been experimented upon felt any pain?—*A.* Only a minority have felt any pain at all; and only a very small minority have I seen which I thought suffered serious pain.

Q. And even in those which you thought felt some pain, that pain was probably not severe?—*A.* No; in the great majority of those which I thought felt any pain, the pain was not severe.

Q. Do you think you have ever seen an animal that was experiencing pain, in consequence of the operation, of the severity, say, of colic, or exhibiting such symptoms as we know animals which are affected with colic display?—*A.* No, I have not seen anything indicating pain so acute as that.

Q. (Mr. Ram) You told Sir John McFadyean that you did not think your visits exercised any restraining influence in preventing illegal experiments?—*A.* Because there was no need of preventing illegal experiments, or preventing people wilfully breaking the law. I have, I think, pre-

vented people breaking the law unwittingly, by informing them of the law. I have on several occasions stopped people who were intending to give ether or cocaine under Certificate A.¹ I have said, "If you mean to do that, you must get Certificate B, or you will be a transgressor of the law"; but I would not put down such a person as a wilful transgressor.

Q. Then, so far as wilful transgression goes, do you attach no importance to inspection?—*A.* No, because the people who hold a licence are all people of high character, so far as I can find out, and selected people; and, as I have said already, if I thought a man would treat the Act lightly, or rather, I should say, treat animals lightly, I would be no party to getting him a licence.

Q. Then apparently, in your opinion, there is very little safeguard against any evil that is procured by inspection?—*A.* I think there is great safeguard. I think the inspection is essential, to secure for the Home Office knowledge of what goes on; and it keeps licensees informed as to what they should do; and, as I have said, it has prevented their stupidly contravening the law. If the character of the people is not such that they would not do wrong, I do not think that any amount of inspection, even staying there all day, would secure that they would behave themselves.

Q. Do you think that the advantages, which in your opinion do accrue from inspection, could be obtained to a larger degree if there were more inspectors, and more frequent inspection?—*A.* No, I am quite satisfied with

¹ "The most frequent irregularity (of which we have one or two instances every year) is, that a licensee holding only Certificate A (or A + E), which allows of inoculations being performed without anæsthetics, administers an anæsthetic to the animal while making the injection."—Mr. Thane, Q. 477.

the amount of inspection. I think it is about the right amount.

Q. You think that to inspect a registered place something like three times a year is sufficient?—*A.* I should say three times a year would be the minimum. But some places I visit have practically no experiments; in one place there has not been an experiment for years, and there is no licensee there at this moment. Then, at other places where work is more active, where there are more licensees and more experiments going on, naturally one visits oftener.

SIR WILLIAM THORNLEY STOKER, M.D., F.R.C.S. IRELAND,
November 14, 1906

SIR W. THORNLEY STOKER, President of the Royal Academy of Medicine in Ireland, and sometime President of the Royal College of Surgeons in Ireland, stated that he had been Inspector for Ireland since 1879. "So far as the Act is concerned," he said, "it has been, I think, carefully administered in Ireland, and properly observed, and exceedingly free from irregularities." He went on to say that, with perhaps very rare exceptions, which he would require to have pointed out to him, he did not think that vivisection was necessary for the illustration of ordinary teaching lectures to students; and he read a memorandum on this point, which he had furnished some time ago to the Chief Secretary for Ireland. In this memorandum, and in his evidence, Sir Thornley Stoker expressed some doubt whether animals were always kept under sufficient anæsthesia throughout a long experiment. But, in his evidence, he said that he had never, on any visit of inspection, seen any experiments beyond inoculations; and that he had not taken any trouble to keep himself informed of recent investigations into the best method of anæsthetising dogs.

He said that at present there were only ten places in Ireland where experiments were carried on. Some of these experiments were made for the Local Government Board; nearly all of these were inoculations. He suggested certain changes as to the licensing authority in Ireland. He was asked his experience as to the humanity and carefulness exercised in experiments on animals, and he answered, "I think that so far as the letter of the Act is concerned, it has been administered and practised with a great deal of humanity in Ireland." He inspected all the registered places twice a year, paying surprise visits; he had never found anything irregular, or to complain of. Very few experiments were made in Ireland by way of demonstration classes. He had always found the condition of the animals satisfactory; they were well cared for, well fed, and clean. He did not know of any experiments being made by unlicensed persons. In 1905, there had been only eight demonstration-experiments in Ireland; six in Belfast and two in Dublin. He did not think there were any abuses existing in Ireland; he did not think there was any concealment, or anything that a dozen more inspectors could find out. Later, he was asked the following question by Dr. Gaskell: "I want to arrive, if I can, at why you consider that there has been any cruelty in these demonstrations before students. I understand that you yourself have never seen any cases, and that you are relying on the evidence of students?—

A. I have seen some research work; I have never been a student of it. But I did not speak of cruelty in the demonstrations. I spoke of the want of necessity, even granting that there is no cruelty. I was quite willing to admit that there was no cruelty. I do not think it is necessary; that is my great point. . . . My broad contention is, that the ordinary student can be sufficiently

well taught without vivisection in illustration of a lecture. I may say that there are schools in Ireland where it is never done, where lectures are not illustrated, and where the professors do not elect to do it." The following question was asked by Dr. Wilson : " I have it in my mind to ask you this question : whether, instead of this very distinguished Association for the Advancement of Medicine by Research, a Committee of the General Medical Council, which consists of members from Ireland, England, and Scotland, should not be made generally responsible for all these researches which are undertaken ostensibly for the prevention or cure of disease, or the extension of physiological knowledge?—*A.* I doubt whether it would work, for two reasons. One is, that the General Medical Council is a considerably overworked body as it is; and I do not see how men of the position that are on it could find more time to give to a function of that sort. And the other is, that I am not sure that the broad constitution of the Council, in the point of not being altogether composed of scientific men, would lend itself well. Many of the men who hold positions on it are chiefly eminent as medical politicians, and so on. I do not know that you could easily get a scientific element there. You would get the advice of a number of eminent, clever, and common-sense men of the world; I doubt if you would get anything beyond that from it as a body, on a point of this sort."

II.

THE RESEARCH DEFENCE SOCIETY

It is thirty years between the two Royal Commissions on Vivisection. During these thirty years, the principles of Pasteur's work have been applied to the daily practice of medicine and surgery, and to every use of preventive and State medicine. The result is, that experiments on animals have come down out of science into every-day affairs. The Government, the County Councils, and other public bodies, all make use of experiments on animals. The State is "the vivisector"; and many experiments on animals are paid for, like the cleaning of the streets, out of the public money. But that is not all. The ratepayer not only supports vivisection, he is also keenly interested in the results which it has achieved. He well understands this plain fact, that animals, no less than men, women, and children, owe a profound debt to experiments on animals; and he likes to hear about Pasteur and Lister, and how the men of science have blotted out Malta fever among our garrison in Malta, and malaria and yellow fever in the Canal Zone, and are fighting plague in India and sleeping sickness in the Uganda Protectorate. He has read of these good works, and he would be glad to hear more about them. Especially, he wants to hear about them at first hand; he wants the men who did the work to tell him all about it.

The men who did the work, therefore, used the present

Royal Commission to tell him all that he desires to know. They were determined that the facts of the case should be set forth in a full, clear, orderly way; so that nobody could fail to see the national importance of these experiments, and the vast legions of human and animal lives that have been saved.

With so much to be told to the public, it was necessary to avoid all overlapping of evidence, and all waste of this opportunity for public instruction. So, when the Royal Commission was appointed, a meeting was held of some sixty or seventy representatives of scientific and medical societies; and at this meeting a small Committee was nominated, with Professor Starling as chairman, to choose witnesses, to submit their names to the Commission, to be in correspondence with the Commission, and to watch and record the whole course of the inquiry. We owe it to Professor Starling's Committee, that the Royal Commission Blue books contain a most excellent and complete history of the Thirty Years' War against disease, written, as it were, by the men who did the fighting.

When the Royal Commission ceased to hear evidence, it seemed a pity that the machinery of Professor Starling's Committee should be broken up. So, on January 27, 1908, at a meeting of that Committee, it was agreed to form a Society, to be called the Research Defence Society. The members of the Committee present at the meeting were Dr. Beevor, Professor Cushny, Dr. Head, Dr. Leonard Hill, Sir Victor Horsley, and Mr. Stephen Paget; also Mr. Sydney Holland, and another good friend of the Research Defence Society, were present. A Committee was formed; Mr. J. Luard Pattisson was elected Hon. Treasurer, Mr. Sydney Holland was elected Chairman of Committee: and Lord Cromer consented to be President. In March, Mr. Pattisson was succeeded by the present Hon. Treasurer,

Dr. F. M. Sandwith. On April 24, the following letter was published in the newspapers :

RESEARCH DEFENCE SOCIETY

SIR,

A Society has been formed, with the name of the Research Defence Society, to make known the facts as to experiments on animals in this country ; the immense importance to the welfare of mankind of such experiments ; and the great saving of human life and health directly attributable to them.

The great advance that has been made during the last quarter of a century in our knowledge of the functions of the body, and of the causes of disease, would have been impossible without a combination of experiment and observation.

The use of antiseptics, and the modern treatment of wounds, is the direct outcome of the experiments of Pasteur and Lister. Pasteur's discovery of the microbial cause of puerperal fever has in itself enormously reduced the deaths of women in child-birth.

The nature of tuberculosis is now known, and its incidence has materially diminished.

We owe the invention of diphtheria antitoxin entirely to experiments on animals.

The causes of plague, cholera, typhoid, Mediterranean fever, and sleeping sickness have been discovered solely by the experimental method.

Not only have a large number of drugs been placed at our disposal, but accurate knowledge has replaced the empirical use of many of those previously known.

The evidence before the Royal Commission has shown that these experiments are conducted with proper care ; the small amount of pain or discomfort inflicted is insignificant compared with the great gain to knowledge and the direct advantage to humanity.

While acknowledging in general the utility of the experimental method, efforts have been made by a section of the public to throw discredit on all experiments involving the use of animals. The Research

Defence Society will therefore endeavour to make it clear that medical and other scientific men who employ these methods are not less humane than the rest of their countrymen, who daily, though perhaps unconsciously, profit by them.

The Society proposes to give information to all enquirers, to publish *précis*, articles, and leaflets, to make arrangements for lectures, to send speakers, if required, to debates, and to assist all who desire to examine the arguments on behalf of experiments on animals. It hopes to establish branches in our chief cities, and thus to be in touch with all parts of the kingdom; and to be at the service of municipal bodies, Hospitals, and other public institutions.

The Society was formed on January 27th of the present year, and already numbers more than 800 members. It is not an association of men of science or of medical men alone; its membership has been drawn from all departments of public life, and includes representatives of every class of educated Englishmen and Englishwomen, including many who have taken an active part in the prevention of cruelty to animals. This fact is in itself a remarkable protest against the attacks which have been made on the researches that the Society has been formed to defend.

The annual subscription is five shillings to cover working expenses; but larger subscriptions, or donations, will be gladly received. The acting Hon. Treasurer, *pro tem.*, is Mr. J. Luard Pattisson, C.B. (of the Lister Institute), and an account in the Society's name has been opened with Messrs. Coutts & Co., 440, Strand. The Hon. Secretary is Mr. Stephen Paget, 70, Harley Street, W., to whom all communications should be addressed.

Yours faithfully,

CROMER

(President).

On June 19, 1908, the Society held its Inaugural Meeting, at the house of the Royal Society of Medicine. Lord

Cromer presided, and delivered an address; the other speakers were Sir Thomas Barlow, Lord Robert Cecil, Mr. Walter Long, Dr. C. J. Martin, Sir Henry Butlin, and Mr. Sydney Holland.

At the present time (July, 1911) the Society has about 4,250 Members and 400 Associates, and has formed twenty-seven Branch Societies.

The Research Defence Society has nothing to do with the actual making of experiments on animals, nor with advising the Home Office over applications for licences or certificates; nor does it desire to see the abolition of all restriction of these experiments.

The *minimum* subscription for members, to cover working expenses, is five shillings; but undergraduates and students of medicine are eligible for membership at a subscription of half a crown. Associates pay a subscription of one shilling. A donation of ten pounds constitutes life-membership. All publications are sent to each member, and all short publications to each associate. The Annual General Meeting is held in London in June.

The Society ought to have a larger number of members and associates: for its work keeps growing, and the expenditure increases with the work. The Committee greatly hope that all members and associates will help them by enlisting more supporters of the Society, and by taking an active part in the advancement of its affairs. All communications should be addressed either to the Hon. Secretary, 21, Ladbroke Square, London, W., or to one of the Hon. Secretaries of the Branch Societies. They will gladly answer all inquiries, supply literature, make arrangements for addresses and lantern-lectures, and receive names for membership or associateship.

III

PHYSIOLOGY

PROFESSOR STARLING, *December 12, 19, 20, 1906*

DR. E. H. STARLING, M.D., F.R.S., Professor of Physiology at University College since 1899, made a general statement as to the objects and the work of physiology. He then proceeded to speak of the character of such experiments. "It is probable," he said, "that many of these subjects would remain uninvestigated, and the advance of our knowledge on these subjects would not have taken place, if the experiments of which I have given examples involved the infliction of pain—or at least, of pain at all severe. This is not the case. The introduction of anæsthetics and new narcotics, and of the aseptic method of operation, into physiology, has well-nigh abolished pain from our physiological laboratories, as it has from the surgical wards of our hospitals. I do not think that the absolutely painless character of the vast majority of physiological experiments is sufficiently appreciated. Records of classical experiments, performed before anæsthetics were invented or had come into general use in laboratories, are too apt to be taken as typical of those of the present day, when the use of anæsthetics is invariable in all experiments more extensive than a simple inoculation. Though I have

been engaged in the experimental pursuit of physiology for the last seventeen years, on no occasion have I ever seen pain inflicted in any experiment on a dog or cat, or, I might add, a rabbit, in a physiological laboratory in this country; and my testimony would be borne out by that of any one engaged in experimental work in this country. It is not, however, merely the normal humanity of the operator that should deter the infliction of pain in a physiological experiment. It is the object of the experimenter to limit the field of his experiment so far as possible, so that when he is, so to speak, putting a question to any function of the body, this function shall be unaffected by any factor other than that which is being controlled by the experimenter. Of all possible disturbing factors in the body, none can be greater than that of pain. It is a common experience that a slight toothache will upset the processes of digestion; and a storm of pain playing on the different functions of the body would make it impossible to judge how far any result obtained was due to our experimental interference, and how much to the regular actions of the pain inflicted. It is true that the anæsthetised condition may be regarded as more or less abnormal. We are able, however, by using different anæsthetics, to vary this abnormality from one experiment to another, and thus to allow for it in interpreting the results of our experiments."

And again: "In the majority of physiological experiments, no pain is inflicted. In a certain small proportion of cases, although we cannot speak of actual pain, the effect of our operative measures may be to cause sickness, followed by the death of the animal. In all such cases the animal must feel ill and miserable, just as it does in distemper. A disease such as diabetes is produced in the animal in order that we may study the conditions on

which it depends, and so learn to control them. Such experiments do not, however, form 1 per cent. of the total number of experiments on dogs. Any legal prohibition, therefore, of the use of dogs for experimental purposes, would deal an irremediable blow to the advance of physiology and medical science in this country; while the only practical result to the dog would be that a few hundreds more would be killed in the lethal chamber at the Battersea Dogs' Home, instead of obtaining euthanasia at the hands of the physiologist."

Professor Starling went on to speak of the value of experimental demonstrations in the teaching of physiology. As examples of such fundamental demonstrations, he mentioned the demonstration of the blood pressure, and its variation under different conditions; the action of the heart, its alteration with lack of oxygen, and its reaction to increased strain thrown upon it by contraction of the arteries; the nervous mechanism of the secretion of saliva; the influence of the normal chemical stimulus on the secretion of pancreatic juice and bile; and the nervous mechanism of the respiratory movements. "None of these experiments," he said, "involve any infliction of pain. The animal is fully anæsthetised throughout, and is killed, while still under the influence of the anæsthetic, at the end of the experiment. The student thereby not only gains a knowledge of physiology to serve as the basis of his future medical studies, but he acquires some idea of the methods of administering anæsthetics, and of the dangers connected therewith. . . . We are going to turn out these men with the power of life and death over their fellow creatures, and therefore one has to employ every means in one's power to give them a knowledge of the workings of the organs of the body which they have to treat. Under the present law, experiments are never performed by students for the

sake of acquiring knowledge ; and, indeed, even surgeons are expressly forbidden to perform operations on animals in order to acquire surgical skill. The student's first administration of an anæsthetic, or his first tracheotomy, is therefore performed on a human patient. On the same priceless material, the surgeon has to acquire the skill necessary for such difficult operations as resection of the stomach and intestines—operations whose possibility was only established by repeated trials by physiologists on lower animals. One can hardly believe that the framers of the present law regarded the life of animals as more sacred than that of man. Under a competent and licensed teacher, a student might with perfect propriety learn the methods of anæsthesia, and study some of the chief functions of the body by experiment on animals under anæsthetics, before beginning his medical studies. A new operation, or one which is new to a surgeon, should most certainly be tried by him, under anæsthetics, and with the same aseptic precautions as would be used on man, on the lower animals. I think the present regulation of the law, which expressly forbids surgeons to acquire skill by experiment in operating on animals, is most immoral. A man must acquire skill somehow, and it means his acquiring skill by experiments on his human patients. The effect of that would be evident if you took half a dozen surgeons, and compared the death-rate of their intestinal resections in the first ten cases and in their second ten cases. You would see what that means in human life."

He gave about eight demonstrations in the course of the year. A demonstration, as a rule, would be a single experiment on one animal. The animals were always anæsthetised, and were killed at the end of the experiment. Asked whether there was anything to prevent "any anti-

vivisectionist doctor" from attending these lectures, he said that there was not.

Professor Starling was then asked as to the use of anæsthetics, and as to the action of curare (see Chapter IX).

Certain statements made by Mrs. Cook (see Chapter X) were then submitted to Professor Starling. He pointed out that Mrs. Cook had failed to note the fact that anæsthetics were thoroughly and completely given throughout the whole of certain experiments described in her statements. In another instance, Mrs. Cook had failed to note the difference between the distal end of a divided nerve, and the proximal end. In another instance, she had made a statement which was absolutely false. In another instance, she had also made a statement which was absolutely false.

Asked about certain experiments in relation to the pathology of gall-stones, he said that practically all the viscera, the liver, the intestines, the spleen, the kidneys, and the ovaries, could be regarded as painless: they had no sense of pain. They could be cut without any pain whatever. They had nerves, but the function of protection of the viscera was entrusted by nature, if one might so express it, to the abdominal wall. It was the deeper layer of the abdominal wall which felt pain, and caused an instant contraction to protect the inflamed organ: but it was not the inflamed organ itself that was painful. The one type of stimulation of the viscera which was painful, was strong contraction, or stretchings: but the same organs could be cut and stitched without giving rise to any pain whatever. Thus, the operation of cutting out a piece of the bowel and sewing the two ends together again, was not painful. Asked about experiments in the withholding of food from animals, he said: "One naturally is inclined to regard starvation as an

experiment necessarily associated with pain ; but we have, in this connection, a number of experiments which physiologists have made on themselves, and we have also experiments which men have made on themselves for purposes of gain. There were two Italian fasters—Cetti and Succi. I think both these men fasted for as long as thirty days ; and physiologists in Germany and elsewhere, especially in Tigerstedt's laboratory, and in Helsingfors, have frequently undergone periods of starvation of from five to seven days ; and it is the universal testimony of these men that starvation is not painful. The effect of starvation is to cause, during the first two days, hunger, which may rise to unpleasantness at meal-times, going-off in between. After the first two days, hunger itself is hardly felt ; the only feeling is one of extreme disinclination to take any form of exercise whatever, and animals which are starved simply lie curled up, and, if roused, drop down back into their torpor again ; and the men, who of course had to take certain pains because they were making experiments on themselves, found that the only unpleasant part was rousing themselves to take observations on their excretions, or their temperature, and so on. Otherwise there was nothing that could be described by them as painful ; and even the unpleasantness, after the first two days, was not of any marked extent." Asked about thirst, he answered : "I believe that thirst is a different thing. I do not know of any experiments which have been made upon thirst ; but I believe there are actual pains there, and there are many stories to that effect, that the deprivation of water is a seriously painful process. But I do not know of any physiological experiments in that connection. In all these starvation experiments, water is given."

Professor Starling was then asked about the "Brown

Dog." He answered: "The case was this. A dog, on which an experiment had been performed, had to be killed under chloroform. The experiment was at an end. The dog was quite healthy; nothing had resulted from the first operative procedure. Of course, it would have been possible to kill that dog, to keep it under chloroform until it died, and then to have taken another dog for another experiment which we were going to make. Instead of killing this dog straight off with chloroform when it was anæsthetised, what was done was, that it was used to show an experiment under anæsthetics, and then it was killed. The operation was done; then the dog lived for a couple of months—I do not quite remember the exact time—and it was quite well: simply nothing had resulted from the first experiment. Then the dog had to be killed, because we wanted to see what had happened: and, as it had to be killed, we used this dog instead of taking a fresh one. . . . The first operation was this: the dog was anæsthetised, the (pancreatic) duct was tied, and then the wound was sewn up and dressed, and then the dog was allowed to recover, and it got quite well. Then it was kept for a long time, because we thought that something might occur in its well-being; but it remained perfectly well. Then the time came to kill it; and, as it had to be killed, we utilised it for an experiment before a class, and then killed it. So that it was merely a question of its being under anæsthesia for a couple of hours, instead of being anæsthetised till it was dead in about a quarter of an hour."

Asked whether he could suggest any alteration in the present Act, he answered: "It seems to me that the whole machinery is rather complex, and does not distinguish between different classes of licences sufficiently. It seems to me, for instance, that instead of this complicated list of different certificates and licences, you might have

simply two classes of licence—a first-class licence, which should cover all licences and certificates, which should be given to people in a responsible position; and then a second limited licence or certificate—it does not matter which you call it—which should be given on the recommendation of the head of the laboratory, and which should apply to a certain research, and should be given to people, whether qualified or not, who, so far as experimentation on living animals is concerned, are still, so to speak, *in statu pupillari*.”

Asked about the signing of applications for licences, he said that he had never signed one without satisfying himself that the applicant had some definite purpose in his experiments. He took every precaution to know that the man for whom he was signing a certificate was a proper sort of person; and he had never signed an open paper. Asked about the experiment of an artificial fistula in dogs, he said that, at first, probably there would be discomfort; later, the animals were quite happy, they were not even in a state of apprehension, they behaved like normal dogs. Asked as to a suggestion that animals were terrified before an experiment, he answered that he had not had any such experience. “These animals know nothing,” he said. “If the animal is wild, it objects to being brought into any room, or being handled at all. If it is not wild, if a dog is brought into the room, it is as pleased to come into that room as any other; there are never any signs of any fright in these animals when present in the room, or that they have any idea of what they are going to suffer.” He had never seen any carelessness or recklessness in his laboratory. He had never seen any omission to kill the animal. All operative procedures in his laboratory took place under anæsthetics. If the ordinary commercial operations on animals were to be done in his laboratory, full anæsthesia

and antiseptic precautions would be employed. In operations in his laboratory, if the antiseptic precautions failed, and the case went wrong, the animal was killed. "So soon as it is found that the case is going wrong, and the animal is ill, and the wound is dressed and found to be suppurating, the animal would be at once put into a box, and chloroformed to death." Asked for any suggestions as to further protection to be given to dogs, he answered: "I cannot see that it is necessary, because I think my evidence has been entirely to show that the dogs are not ill-treated, and do not suffer under the vast majority of the experiments which are made upon them. Not only do we take precautions to prevent pain, but we take precautions even to prevent fright or discomfort to the animal."

Asked to define "pure curiosity" as a reason for experiments on animals, he answered, "What I say is, that the desire to advance physiological knowledge is this spirit of curiosity, which I am trying to show is a right spirit. When you apply for a certificate, you apply for a special manifestation of this spirit of pure curiosity—namely, curiosity as to why, for instance, the salivary glands secrete." The certificate was given for a special object, and the object was defined. Asked about the killing of any animal in a state of serious suffering after an experiment, he said that he should certainly order it to be killed, even if that would spoil the result of his experiment. The animal ought to be killed at once, and it would be so killed. Asked what would come of the prohibition of Certificate B in experiments on dogs, he said that it would stop all the more advanced observations on digestion, experiments as to the nature of diabetes, and other important cases. Asked about experiments for the purpose of demonstration, he answered, "Of course we can teach

school-teachers physiology out of text-books and by diagrams, and we do, and it is sufficient knowledge for those who want a smattering of it; but for men who will have the handling of every organ in the body, it is not enough that they should have simply a word-knowledge derived from books. I say that it is necessary that they should see experiments performed on living animals."

Asked whether he would be averse to increased inspection, he said "Not at all." There might be a hundred inspectors, so far as the licensees were concerned. Asked about the popular phrase, "the closed doors of the laboratory," he answered, "I have never come across a laboratory where there were any closed doors. In my laboratory, any student wanting to speak to me walks straight in. The door of the laboratory where I do the chief part of my work is always open to the passage, because the ventilation is not very good, and they can walk straight in and talk to me as I carry out the experiment." He considered that the present Act had allowed good work to be done, and had at the same time protected animals. He did not wish to see it abrogated, or altered to any very marked extent. Asked about the keeping alive of animals for observation after such an operation as removal of a portion of the bowel, he answered: "If the operation is quite successful, the animal will not be in what you might call a state of pain. There would be a little soreness in the abdominal wound, but it would be so slight that in the cases I have had, in the greater number at any rate, the animal seems to be unconscious of it. For instance, it stands up and puts its paws against the cage, stretching the abdominal wall without apparently feeling anything at all in the wound; so that I should say that pain was quite absent. On the other hand, if the operation were unsuccessful, there would be a certain amount of pain,

because there would probably be some inflammation round the abdomen; peritonitis would be caused, and that would probably be attended by pain. In such a case, as soon as the animal seemed to have peritonitis it would be killed; but before it was discovered it might be in pain for a few hours."

Asked, as a matter of opinion and feeling, whether, if there were no such things as anæsthetics, he would justify painful experiments upon animals, he answered "Yes. The condition would be very much like that of surgery before the discovery of anæsthesia, when only a few men became surgeons, and surgery was much limited by the fact of the enormous amount of pain connected with its practice. There is no doubt that physiology would be practised, and would be justifiably practised, in the absence of anæsthetics; but, if our humane feelings had grown as they have, without the invention of anæsthetics, there is no doubt that the pursuit of physiology would be an extremely restricted one, and would be limited to those men who had the moral courage to carry out those experiments. Such men would be worthy of admiration rather than of condemnation."

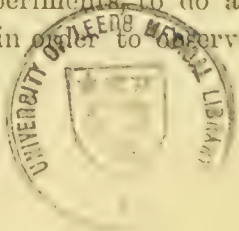
Asked about the use of dogs for experiment, he said that they were very rarely used for inoculation. Asked about the use of them for experiments in physiology, he explained that the physiologist needed to be able to take into account abnormal conditions. Thus, to remove the pancreas was to run this great risk of abnormality. It was a condition which one would not produce without proper cause for it; but to establish an intestinal fistula did not make a dog abnormal in any way, nor did the animal suffer pain from the fistula.

PROFESSOR SCHÄFER, *June 12, 1907*

Dr. E. A. Schäfer, LL.D., Sc.D., F.R.S., Professor of Physiology in the University of Edinburgh, read a statement from the Royal Society of Edinburgh, as to the value and the necessity of experiments on animals. Asked as to demonstrations to students, he said, "I not only approve of the practice, but I do not think it is possible to teach physiology properly without showing a certain number of experiments to students." He gave as instances certain facts concerning circulation, blood-pressure, the heart's action, secretion, and cerebral localisation. These experiments required only a very small number of animals. Such experiments excited neither horror nor levity among students. "I have now been teaching physiology in one capacity or another for thirty-seven years, and I have a very large experience of classes of physiology, and a very large experience of demonstrations and experiments; and I have never in a single instance found that there was the least tendency to levity when an animal was under experimentation." Asked whether experiments ought to be permitted for the purpose of gaining skill in operation, he said that he had formed a very decided opinion that it was in the highest degree desirable that any new operation should, if possible, be performed upon a suitable animal.

He was then asked about anæsthetics (see Chapter IX).

Asked about his experiments to determine exactly what happens during death by drowning, he answered as follows: "All except two (and the total number, I think, was thirty-six) were conducted under the influence of complete anæsthesia during the whole time of the experiment; but it was of the highest importance, that one could appraise the value of these experiments, to do a certain number of control experiments, in order to observe whether, so far as



could be determined, the phenomena would be the same without an anæsthetic as with an anæsthetic. I therefore got permission from the Home Secretary to do ten experiments without anæsthetics, and I did two of them. The results which were obtained with those two showed so conclusively that the anæsthetic did not invalidate the object of the experiment, that I left the other eight experiments, and did not perform them at all: and these are the two to which I refer. In these two experiments, the animals were simply drowned by being held under water and not allowed to recover at all: and the obvious phenomena, such as the pulse and respiration, were observed, and a post-mortem examination was made in order to see whether the post-mortem conditions were the same when they were drowned without any anæsthetic, as with an anæsthetic." From these experiments he had discovered a better method of performing artificial respiration in cases of drowning. This method was easier to teach, and easier to practise, and gave a much greater respiration, than either Sylvester's method or Marshall Hall's method.

Asked about experiments on cerebral localisation, he said that the analogy between a monkey's brain and a human brain, with regard to the motor area, was fairly complete, as had been shown by cases in which the brain had been exposed in man. In such cases, it had been repeatedly found that excitation of the same or the corresponding part of the brain in man to that which yielded a certain result in a monkey, would give the same result. That had been repeatedly shown in man. In all these experiments on monkeys, there was no sign of pain after the operation—absolutely none. "The animal is in a perfectly normal condition, eats its food, and runs about the cage, except for such paralysis as we may have produced as the result of the operation. If there was any sign of

pain, we should immediately kill the animal ; but we have never had occasion to do this since the introduction of antiseptic methods. You would be surprised to see how well these animals are and look."

Asked about antiseptic and aseptic methods, he said that Pasteur's experiments upon fermentation had led directly to the whole of "Listerism." The use of the spray, now given up, was a mere detail. Aseptic surgery was the most complete carrying out of Listerism. In antiseptic and aseptic methods, the principle was the same. "The principle is the exclusion of germs. In one case you kill them, and in the other case you prevent their entrance ; but the essential principle of Listerism is the exclusion of germs. There is no question about it, I should think."

Professor Schäfer was then asked whether he would advocate any change in the law as at present administered or stated in the Act : and he read a statement, referring to undue delay in the allowing of certificates, and to certain actions by the Home Office outside the text of the Act. He went on to speak of the harm that would be done by any increase of restriction :

"I would strongly deprecate, on behalf both of the Royal Society of Edinburgh and of the University of Edinburgh, the imposition of any further restrictions whatever. Any restrictions on scientific work are detrimental to its progress, and further restrictions of experiments on animals might prove disastrous to the progress of physiology and medical science in this country. Since the revival of scientific medicine in the last two centuries, we have taken a leading part in contributing to its progress. In no other country in the world is more regard paid to the ethical aspect of the treatment of animals ; and the care which has always been exercised, by physiologists in this country, to prevent pain in the performance of experi-

ments, is a reflection of a general sentiment which animates all classes of society. (The callousness of the laity generally, with regard to the sufferings which are inflicted upon animals intended for the table, or which are involved in pursuit of pleasure, is due, I believe, in the majority of cases, to sheer ignorance.) Nevertheless, I have not found any disregard of animal suffering to prevail amongst physiologists and medical men of other countries, in spite of the fact that in some countries the people generally are far more callous regarding animals than are we ourselves. It is strange that this should be the only country in the world in which restrictions are placed on the use of animals for the purpose of advancing medical and scientific knowledge, in spite of the fact that there is overwhelming evidence that the actual amount of pain inflicted is infinitesimal compared with the results achieved, and compared with that which is daily and hourly inflicted in the pursuit of pleasure or of commerce. Even quite recently, a clause was inserted in an Act of Parliament, prohibiting stray and unclaimed dogs seized by the police being employed for the advancement of medical knowledge. The silliness of such a regulation is self-apparent; for these animals are condemned to die, and are in many cases killed by a process of suffocation; at the best, they are killed by the same anæsthetic as would be employed in the laboratory; and the result is, that other dogs have to be killed in the laboratory, and the number of animals sacrificed is increased by so many. Who benefits by this? Certainly not the canine race.

“The necessity of employing dogs has been emphasised by others who have given evidence, but it cannot be too strongly insisted upon. No other animals are available for experiments in important fields of physiology. Among these I would instance experiments upon the heart and

circulation, upon the secreting glands and the digestive system, upon the kidneys and the formation of the constituents of urine, upon lymph formation, upon metabolism, and lastly experiments on the nervous system, in certain of which animals which can be trained must be employed. Dogs are the only animals which are available for many such experiments."

He agreed that animals, after an experiment under Certificate B, ought to be kept on a farm, not in a laboratory in London. He also agreed that the making of inoculations might be done by assistants, under a licence held by the head of the laboratory. He would be inclined to confine the Act to warm-blooded animals.

Asked as to the allegation that the sight of experiments on animals had a demoralising influence on students, he answered, from thirty-seven years' experience as a teacher, that it was absolutely without foundation. "The men I know best are the men who have been my assistants, who have been engaged, as students and as teachers, in operating upon animals. I should say that amongst all my friends there are none more humane than those who are constantly engaged in experimenting upon animals. So far as the effect on students is concerned, it is obvious that what you have mentioned cannot be the case. The great mass of medical men who are turned out nowadays have seen experiments on animals—they certainly have at Edinburgh, for my predecessor showed many more experiments than I do, and hundreds and thousands of students passed through his hands—and I have not heard that the Edinburgh medical graduate is more callous than any other medical man who has not seen experiments on animals." During his thirty-seven years' teaching, no animal under experiment had ever made purposive movements, or struggled, or cried under the anæsthetic. "Patients taking chloroform, al-

though they are perfectly without sensation, make all sorts of noises, sing, scream, and make ridiculous utterances, without being the least conscious of it, and without having any memory of having done so when they wake up. And, in the same way, one of the symptoms which is observed, when a dog is put into a box or given chloroform, is, just before he has become profoundly anæsthetised, a succession of loud noises, not exactly a bark, not exactly a whine, and not exactly a howl, but peculiar noises which might be taken by anybody, who was not an expert on the subject, to indicate that the animal is under the influence of pain. It is nothing of the sort at all. It belongs to the same class of noise that patients make who sing and cry out when they are going under chloroform, just before they are completely under its influence. Some dogs are perfectly quiet, and others make a great deal of noise. As a matter of fact, the animal is simply in a box with vapour of chloroform being pumped into it. In my experience, dogs are more easy to anæsthetise than any other animal."

Asked whether he wished to bring anything else before the Commissioners, Professor Schäfer answered: "Something has been said about the importance of clinical observation, and as to the possibility of clinical observation superseding experiments altogether. I should like to illustrate the defects of mere clinical observation, unassisted by experimentation on animals, by a statement regarding the discovery of the active principle of supra-renal extract. Dr. George Oliver had been making a large number of clinical observations upon the effect of various organ-extracts upon the circulation, but had been unable to arrive at any definite conclusions regarding them. Amongst these, was extract of supra-renal capsule, extract of thyroid gland, extract of brain, and so on. He consulted me as to what steps might be taken to arrive at a clearer

understanding with regard to their action: and I invited him to investigate their physiological action along with me upon animals in the laboratory. This we proceeded to do; and the result of the investigation was, that the majority of the extracts from which he supposed that he had obtained definite results in man gave no indications of physiological activity: whereas, on the other hand, the extract of supra-renal capsule gave such manifest indications of activity that it was quite clear a very important principle was contained within this organ. The properties of this principle we then proceeded to work out: and the result of the investigation led to the discovery of adrenalin, which has proved of immense physiological, and will in all probability prove of great therapeutical and clinical importance."

PROFESSOR GOTCH, *October 30, 1907*

Dr. Francis Gotch, D.Sc., F.R.S., Waynflete Professor of Physiology in the University of Oxford, made a prefatory statement, emphasising the fact that physiology and pathology are practically two branches of the same science, and that each of them owes a very great debt to physics, chemistry, and biology. "This debt they partially repay by the fact that physiological and pathological discoveries and methods have in their turn helped the advance of these other sciences." He gave instances of the necessity of the experimental method in physiology, and of the importance of experiments for the proper teaching of physiology. The phenomena most essential for students to observe were the blood-pressure, the working of the heart, the intra-thoracic pressure and its changes during respiration, the respiratory movements, the action of such substances as raise and lower the blood-pressure, the reflex control exercised by the nervous system over the circulation and the respiration,

the control of the nervous system over secretion and the digestive organs, and, finally, the localisation of reflex motor centres in special parts of the central nervous system.

Asked as to certain allegations of levity among medical students during such demonstrations, he answered: "On the contrary, I am always impressed by the circumstance that this has been the most serious part, and is regarded as the most serious part, of practical physiological teaching."

Asked as to the choice of an anæsthetic, he answered: "I hold that the demonstration of the anæsthetising influence of chloroform and ether, singly or mixed, itself presents phenomena of the greatest practical importance to medical students. Apart from this, I am inclined to use, in my own demonstrations, which are made upon rabbits, chloral-hydrate, or urethane, which, introduced some time previously in the alimentary canal in sufficient quantity, causes a profound and increasingly developed anæsthesia: the animal's condition gradually passes beyond anæsthesia into painless death." He thought that students, in their last year at the University, might perhaps be allowed, under skilled and authorised supervision, and guarded by careful regulations, to make, on profoundly and lethally anæsthetised animals, some of the experimental procedures of demonstration. He made this suggestion because of the great responsibility of medical, and particularly of surgical, practice.

Asked about certain allegations made against his department by some undergraduates at Keble College, he explained the fact, and read letters from the Vice-Chancellor of the University and from the Warden of Keble, stating that the undergraduates had made him a complete apology, with an explicit and unqualified withdrawal of their letters and statements.

Asked to define the "pithing" of a frog, he said that he always used the term for the destruction of the whole central nervous system, the spinal cord as well as the brain. He did not know a single physiologist of the last thirty years who would say that a cold-blooded animal, after destruction of the cerebral hemispheres, could feel pain.

He then made some suggestions in regard to the working of the Act. He thought that the Act ought to be limited to warm-blooded animals. The use of cold-blooded animals should come under the ordinary law of cruelty to animals. Asked about inspection, he said that he was visited two or three or four times a year. Asked whether he had any objection to the inspectors being multiplied in numbers, and their visits being multiplied, he said, "Not at all." Asked about the phrase "initial operation" in Certificate B, he said it covered the whole period till the wound was closed and dressed; and every physiologist that he knew of gave that interpretation to the term. He agreed that it would be an immense advantage, not only for science, but for the animals themselves, if animals could be kept, after operation under Certificate B, not in laboratories, but in outside farms. He was asked, "You know most of the physiologists, or I might say all the physiologists in England practically: have you any evidence whether or no they are kind-hearted men, who are fond of animals?" and he answered, "I think it is rather a remarkable thing that all physiologists almost are fond of animals; and most of them, so far as dogs are concerned, are particularly attached to dogs, and keep them as pets."

DR. PEMBREY, *October 30, 1907*

Dr. M. S. Pembrey, M.D., Lecturer in Physiology at Guy's Hospital, was asked about a statement made by Miss

Lind-af-Hageby, in her book, "The Shambles of Science," as follows: "We once saw a marmot, the spinal cord of which had previously been divided by the vivisector"; and it was explained to him that this was rather a principal point of his evidence. He answered that the statement was absolutely untrue; he had brought the marmot's body with him, and he offered to dissect it then and there, to show that the spinal cord had never been touched. No experiment had ever been performed on the marmot, except that it had been kept on some occasions without food, and its temperature and weight and general conditions had been observed during waking and during hibernation. Hibernating animals often took no food for days in the summer, and in the winter they would go without food for six months. The allegation about the marmot's spinal cord arose out of the natural transient loss of power in the hind legs when the animal was waking up from hibernation. By the time that the animal got back from London University to Guy's Hospital, there was no paralysis at all. The marmot was kept as a pet for five years, and died in 1903 of an abscess in its throat.

Dr. Pembrey next referred to an allegation that he had frozen hibernating animals by exposing them to a temperature of ten degrees. He explained that these were degrees Centigrade, which was the ordinary temperature of his room. He then stated that certain other suggestions made by Miss Lind-af-Hageby in her book were not in accordance with facts. He had not put a certain rabbit in a freezing machine, and the statements about that rabbit were false. The rabbit's temperature had not gone down one degree.

Dr. Pembrey then referred to some experiments, made on rats, to find out, for the Local Government Board,

the minimum dose of sulphur dioxide necessary to destroy rats on ships. A rat-catcher on the Thames could employ sulphur dioxide, and could be paid for using it, and could not be interfered with for the destruction of rats in that way. The physiologist was in a worse position than the rat-catcher; for the rat-catcher could kill rats by the thousand, but Dr. Pembrey was limited to experimenting on only five or six. These experiments gave pain to the rats; he had returned them as painful experiments; and these were the painful experiments which he had done.¹

Dr. Pembrey then referred to some experiments which he had made, in Germany, on the influence of bleeding and transfusion. These experiments were directed to see what would be the difference, during bleeding and transfusion, in respiration; and the rabbits were not anæsthetised at all. They were simply tied down, and they did not show any violent struggling or signs of pain. He had brought two

¹ The following letter from Dr. Pembrey was published in November 1910. Many allusions have been made, in the interests of the anti-vivisection cause, to Dr. Pembrey's statement to the Royal Commission that he had made painful experiments and was not ashamed of them; and the public ought to know that he was referring to these experiments on rats, made on behalf of the Local Government Board:

DEAR SIR,—I pay no attention to the statements of the anti-vivisectionists about my evidence before the Royal Commission: it is characteristic that they quote only a part of the evidence. The experiments relate to the use of sulphur dioxide in the destruction of rats in plague-ships. A physiologist can only make such experiments after obtaining a licence, a rat-catcher requires no permit for using the same method in the destruction of rats.

Yours truly,

M. S. PEMBREY.

(See *Southport Visitor*, November 15, 1910.)

rabbits with him, and would like to show the Commissioners that if an animal were bound down on its back, it often passed into a condition of hypnotism; he could take a perfectly intact rabbit, simply put it down on its back, keep a little pressure on it for a short time, then take away the pressure, and the animal would pass into a condition of hypnotism. "Would you like me," he said, "to do the experiment? There is not the least sign of pain." *Chairman*: "Personally, I do not desire it. There are three members of the medical profession present, who understand those things better than I do. I would like to know what they think about it." *Sir William Collins*: "It is not new to me." *Dr. Wilson*: "No; we may accept the statement." *Dr. Gaskell*: "Certainly."

Dr. Pembrey went on to speak of pain as a protective mechanism, as in cases of fracture, abdominal injury, and childbirth. "This is an argument," he said, "to show that a common-sense view should be taken of this question, and that pain must be admitted. I admit that I have done painful experiments, and I am not ashamed of admitting it. They are absolutely necessary. I want to show that pain is part of the scheme of nature, and that we must recognise its existence." It was pointed out to him, by the Chairman, that pain inflicted on an animal, during an experiment on that animal, was not, in any way, protective to the animal. Dr. Pembrey said that he thought a recognised physiologist ought to be given a licence to cover all experiments, without any conditions at all; but that he should, of course, be subject to inspection. He said that he lived in the country, and, as a farmer, if he had lambs he could castrate them without anæsthetics; it would be absurd to give them anæsthetics. "Of course," he said, "as regards pain, one causes very much more pain in the country, even uncon-

sciously, than one does in a physiological laboratory. The worst pain that I ever caused, I am convinced—I could bring evidence in support of it, but I am quite convinced—was in a case where I took away a calf from a cow. The cow was exceedingly fond of the calf; the calf was too big, and I wanted the milk for my children, so I took away the calf and sold it. For two days that cow was in obvious pain: it would not take its food; it went about bellowing, roaring, and moaning, and was so bad that the cottagers complained. There is a case in which it was perfectly obvious to me that the animal was suffering pain for two days. Never in any physiological experiment have I caused so much pain, even under the Act, as one does every day in the country.” He could not see the difference between physical and mental pain, from a physiological point of view.

The following questions were then put by the Chairman:

Q. What I put to you is this. If you inflict pain upon an animal, and you want to do an experiment upon it, you do not do it to protect the animal against anything. The pain that you cause is incidental to your making the experiment.

A. Taking into account all the conditions which you mention, the reactions are not actually protective: but, in ordinary conditions, if an animal were to get such a cut as that, and it were kept alive, the reflex reactions would then be protective.

Q. But the pain that you would inflict would protect no animal?

A. In that individual case it would not be protective, unless the pain was enough to produce syncope.

Q. It would be protective against something that it would not suffer unless you caused it?

A. But I say that even if I caused it, it might produce syncope, so that the animal would not suffer pain.¹

Later, the following question was put and answered :

Q. Do you think it is right to inflict pain upon a healthy animal—I mean, when there is no question of protecting mechanism—when you can discover all that you want to discover if that animal is under anæsthetics ?

A. What I should say to that is exactly this. I will be perfectly straightforward. I say that you should not inflict pain if you can obtain the knowledge in any other way. But I say that even where there is an operation, the pain there is of a protective nature: it may produce syncope, and therefore less sensation of pain. Further, I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of hypnotism, which I offered to show to the Commissioners. That, I think, is one very important point. These animals pass into a condition, so far as one can see, comparable to hypnotism. If you give them anæsthetics, you are introducing a complication which you could remove; and therefore without anæsthetics you actually save life and actually diminish the infliction of pain.

“There is no doubt,” Dr. Pembrey said, “that the excessive use of anæsthetics is a modern danger. In proof of that, is the death-roll from anæsthetics. I have seen cases of people dying—of children dying from an anæsthetic given for simple operations like circumcision. I have circumcised children without anæsthetics. If they cry it does not matter: it is much better that a child should

¹ These answers have been used, in a letter to *The Times*, to suggest that Dr. Pembrey would be willing deliberately to inflict pain, during a physiological experiment, till the animal fainted. (*Times*, Nov. 1, 1910.)

cry and moan than that its life should be lost. That circumcision is not such a serious operation, is shown by the fact that the ordinary medical man does circumcision under anæsthetics, and the Jewish non-medical man does it without anæsthetics. There is an exact case in point, in which the introduction of anæsthetics has done a considerable amount of harm. Take also the case that I gave you of midwifery. The pain that a woman suffers in childbirth is protective. Extend the analogy of the use of anæsthesia, and every woman in childbirth ought to be given an anæsthetic. What would be the result? You would make the nation more degenerate, if possible, than it is at present."

PROFESSOR LANGLEY, *November 12, 1907*

Dr. J. N. Langley, D.Sc., F.R.S., Professor of Physiology in the University of Cambridge, began by speaking of his personal experience in Cambridge. "I have had experience of the physiological laboratory in Cambridge ever since it was founded, and in that time all the experiments within my knowledge have been performed under the influence of anæsthetics. There are two classes of experiments which are carried out. The first are those simply under a licence, in which anæsthesia is always maintained throughout the whole of the experiment, and the animal is killed. In those I do not see how, short of gross carelessness, there can be any pain inflicted: and such carelessness, in my experience, does not occur. The other class of experiments come under a certificate. An anæsthetic is given, and then the animal is allowed to recover from the anæsthetics. The pain inflicted is that which occurs in a surgical operation; the experiment is done under the aseptic treatment, and it is done, moreover, on an animal

that is healthy. An animal that is healthy recovers from an ordinary skin wound or a surgical operation with extraordinary rapidity. I myself have seen animals, in an hour or two after the skin has been sewn up, take their food in the ordinary way, walk about perfectly content ; cats will purr. What I would call the more ordinary physiological experiment, which comes under this certificate, really does not inflict anything that can be called pain. These are the more frequent experiments. Those that are severe I should put under the second class, which I would now speak of.

“ In some of those experiments, possibly—such, for example, as where there is a section of the spinal cord—undoubtedly there is some pain during the process of recovery. That pain is less, I think, than in a human being ; but it does occur, and is of the same nature as in man. So that what I mean, with regard to the class made under the certificate, is, in the first place, that the great majority are quickly recovered from, and do not inflict pain, although they come under this certificate which implies that they do. Then, secondly, there are some operations which do inflict pain, but the pain in that case is never, I think, as great as is common in a surgical operation on man. And on the whole, so far as my experience has gone in the physiological laboratory at Cambridge, the total amount of pain that has been inflicted is exceedingly slight. There are very few cases where I could have said that the animal had been suffering pain after any operation where the anæsthesia under the certificate has been allowed to cease. I may also mention that during the whole surgical part of the operation the anæsthesia is strictly maintained ; that there is no question about doing anything at all on the animal unless it is anæsthetised, and properly anæsthetised. All the skin stitches, for example,

are inserted under anæsthesia; nothing is done to an animal except in that condition. Everything that is done to the animal is done under anæsthetics. It is then put aside, and after everything is over it slowly recovers from the anæsthetic. In fact, generally, myself I have given a little morphia or a little chloral or something, to prevent it from coming-to until some time after the operation; because an animal will stand more anæsthetic than a man. If it dies, there is not the same serious mischief done, of course; so that on the whole one really gives more and prolongs the anæsthetic more in an animal than one does in man."

Asked as to the duration of anæsthesia during an experiment, he said that he had never seen any sign of a tendency to recovery of consciousness in consequence of lapse of time: generally it was the other way, that the anæsthesia became deeper and deeper. He had seen complete anæsthesia maintained, without artificial respiration, for something between eight and ten hours. Asked about morphia, he said that the animal, in those cases where he had used morphia, lay perfectly still. When severe things like cutting the skin were performed, they did not cause any movement. "I purposely tried this, in consequence of a doubt which has been thrown out with regard to the anæsthetic properties of morphia. This was an animal that was not fastened, so that there could be no question that if it wanted to get up and run away it could do so: and there was no movement." In animals, the limit of range between the disappearance of pain and the disappearance of reflex action was far wider than in man. Chloroform first abolished pain; then it abolished consciousness; then it abolished reflex action; finally, it abolished the reflex action of respiration. Morphia was immensely more selective: it picked

out the pain centre, and it left the lower mechanisms for a time practically intact. "Moreover, it not only does that, but it stimulates a certain part of these lower centres, quite special ones, and they are those which do not give rise to pain in normal life. In an animal under morphia, this selective action can be shown (it is obvious) in this kind of way. If a small point of the skin is taken between the fingers and pinched hard, nothing results; if a knife is taken and the skin is cut, there is no movement; but if you give a loud rap on the table close to it, causing vibration, then there will be a more or less convulsive start of the whole animal. That is not a painful sensation." Similar movements occurred in cases of men dying from an overdose of morphia, and profoundly unconscious to pain. "There are many people who think that when, in the account of an experiment, it is said that the animal moved, or that the animal struggled, it implies a return to consciousness. That is not the case. It is perfectly certain that all the varied kinds of reflex movements, and the general movements of the body, may be noticed long after all pain and consciousness have gone." Especially, slight rhythmic movements of the hind limbs might be produced by slight tension when an animal was fastened in one position under profound anæsthesia. As examples of the value of experiments on animals, Dr. Langley took the study of the vaso-constrictor and vaso-dilator nerves; and the surgical treatment of diseases of nerves, and of severed nerves, with special reference to the operations for cases of facial paralysis, birth-palsy, and athetosis.

The witness was then asked about curare (see Chapter IX).

Asked about the use of dogs, he answered: "In all experiments on animals, of course, one would naturally prefer, for various reasons, sentimental reasons, to avoid the use

of higher mammalia. The same sentiment which makes one hesitate to experiment at all, applies with increasing stress the higher you go in the scale of animals, ending, of course, in man himself ; but experiments on dogs, I think, sometimes are absolutely necessary. Of course, on general grounds, it is clear that physiological laws must have a broader basis, the more different kinds of animals are investigated : and to remove dogs from the category would be to limit the general application and determination of physiological laws." He gave, as instances, two series of observations on the vaso-dilator nerves of the sympathetic system.

Asked about experiments used for demonstration, he answered : " I approve most strongly of giving a certain number of demonstrations to students. I think they ought to see, in the first place, an experiment, under anæsthesia, so that they may judge of it themselves, and form a clearer idea of the working of the body. They cannot see it in any other way." Students took a serious interest in the demonstration ; and he did not understand how any one who had been familiar with students attending such demonstrations could say that they had any demoralising influence whatever. Asked whether he had seen any symptoms of such influence at Cambridge, he answered : " Absolutely none. I think it is quite the reverse ; that they are all extremely anxious to assure themselves that the animal which is there has no trace of pain. One of the things which I generally show in a course is some of these reflexes during anæsthesia ; because, as a rule, they are not satisfied, on the first view, that the movements can be carried out in a wholly anæsthetised animal. They always press home the point, and that is usually shown to them in such a manner that they can satisfactorily convince themselves of it. I am sure that there is no levity of

demeanour; that they are extremely impressed with the necessity of avoiding the infliction of any unnecessary pain. Any exhibition of levity would, I think, be impossible."

Asked for suggestions for the amendment of the Act, he said that he thought animals, after an operation under anæsthesia, if they were allowed to recover, should be kept not in town, but in fresher air. "I have known people, for instance, who would like to take them home, and see after them there, but they are not allowed to do it." He thought, also, that an animal under anæsthesia, before being put to death under the anæsthetic, might rightly be used for another experiment; in that way, one animal would be used instead of two. Asked, whether he thought the Act itself undesirable, he answered: "I would much rather have the Act than no Act. I think it would not be fair to the animals to allow any one to experiment upon them without control. I think it is a very natural desire that there should be some control of experiments which might, if carelessly conducted, inflict pain; and I thoroughly approve of there being some control." He thought that the difficulty and delay of getting certificates had, to some extent, interfered with physiological research; but he certainly did not think that the interference had been really serious. Physiology had, in fact, advanced very greatly in England during the operation of the Act, and, he believed, with a diminution in the amount of pain which had been inflicted.

He was asked his opinion of the suggestion that hypnotism might be employed instead of anæsthetics on animals. He answered, "I have done a good deal of hypnotism, but I never succeeded in hypnotising an animal or a lunatic. You can produce a condition, which is similar to that of hypnotism in man, in animals; but, in an animal,

that condition cannot be maintained for more than a short time. In certain animals it cannot be maintained for more than a few minutes, and it is always uncertain; you could never rely upon keeping it up. It is not a condition in which you could operate on an animal. In a dog, so far as I know, it has never been produced at all. It is done with pigeons, and with other birds, and with frogs; but no serious operation could, in my opinion, be performed on an animal in a state of hypnotism. I should be extremely interested to see the condition of an animal where any one would suggest that you could really substitute hypnotism for anæsthesia."

Asked what he did if he found an animal suffering after recovery under Certificate B, he answered that the animal was killed. Asked as to the suggestion that certain animals, having more acute sight and hearing than human beings, had also a more acute sense of pain, he answered: "Everyday experience of the behaviour of animals which have large gashes and skin wounds is directly opposite to that. I should say that that is a question on which every one can form his own opinion."

Asked about pain inflicted under Certificate B, he answered that he had never seen a case of severe continuous pain; if he found such a case he would put the animal out of its pain. Asked about inspection, he said that he had no objection whatever to increased inspection.

He was then asked about a statement made by Mrs. Cook that he had cruelly tortured a dog. He answered: "That is an example of the misrepresentation on which so much of the anti-vivisection opposition has been founded. There was absolutely no torture; the animal was perfectly quiet, and entirely unconscious throughout."

Asked about another statement by Mrs. Cook, of a similar kind, as to an experiment made by him on a cat, he answered that during the whole of the operation the animal was absolutely unconscious.

Asked whether he thought that cold-blooded animals should be excluded from the Act, he said that he thought they should be put under a special certificate.

IV

MEDICINE

SIR WILLIAM HENRY POWER,¹ K.C.B., F.R.S.,

February 20, 1907

SIR WILLIAM POWER, Medical Officer of the Local Government Board since January 1900, stated that researches involving experiments on animals, made for the Local Government Board, were entrusted as "piecework" to special investigators of highly trained intelligence and approved competence: they were not carried out by the Board's Inspectors. He handed in the scheme of work for 1901-2 (the first year of his more direct responsibility in these matters). This scheme, he said, served to illustrate the considerations which the Board were accustomed to hold in view, and the means taken to give effect to them. The scheme was as follows:

YEAR 1901-2

Research for Administrative Purposes

A. To ascertain whether carbonic oxide (the after-damp of mines) may be utilised for the destruction in

¹ Sir W. H. Power, at the time when he gave evidence before the Royal Commission, was Principal Medical Officer to the Local Government Board: he is also Chairman of the Royal Commission on Tuberculosis, and a Member of the Royal Commission on Sewage Disposal.

ships' holds of rats infesting vessels' cargoes; especially what are the obstacles, if any, to the application of such method of killing rats to vessels of large size, having their cargoes *in situ*. *Result*: "Report by Dr. Haldane on the Application of Carbonic Oxide to the Destruction of Rats on Plague-infested Vessels." (Animal experiment, nil.)

B. Identification and differentiation *bacterioscopically* (apart from the animal body) of particular microbes, in view of need for a standard test in this matter, having a sufficient basis of authority. (a) Investigation of anaërobic microbes which occur in sewage, in water, and in food-stuffs, with a view to ready differentiation of those pathogenic (to man) and those innocent. *Result*: "Report by Dr. Klein on the Differentiation of the several Anaërobic Microbes commonly present in the Intestinal Contents of Man and Other Animals." (Animal experiment on guinea-pigs.) (b) Study of excremental aerobic *B. coli* derived from various sources, with a view to determining strains recently in association with *B. typhosus* and for the purpose of obtaining an index as to the presence or absence in any material of the microbic cause of enteric fever, which is so difficult of detection. *Result*: "Report by Dr. Klein on Agglutination by Blood of Emulsions of Microbes, with special reference to specificity." (Animal experiment on rabbits and guinea-pigs.) (c) Search among bacilli liable to inhabit the throat of the human subject for means of readily distinguishing bacteria which are derivatively and potentially diphtheria bacilli from those other bacteria which, though non-pathogenic, have been confounded with the microbic cause of diphtheria. *Result*: "Report by Dr. Mervyn Gordon on *Bacillus diphtherie* and micro-organisms liable to be confounded therewith." (Animal experiment on guinea-pigs.)

Research for Prophylactic Purposes

Plague, as well as enteric fever, threatened to call for preventive inoculation of individuals. For plague,

the preventive then available was Haffkine's artificial culture of *B. pestis*, plus the products of these bacilli, sterilised. For enteric fever there was Wright's cultivation of the "typhoid bacillus." Neither had proved altogether satisfactory. Against Haffkine's cultures it had been urged that they comprised in uncertain amounts two principles: the one bound up with the bodies of the bacilli, the other an element of their chemical products, and that the virtue of the one and the other principle was not separately known. Against the enteric fever prophylactic was the fact that it had not uncommonly been found less potent for its purpose than is desirable, and that a more trustworthy preventive of the disease was being demanded. Hence (a) investigation of the nature and their effects on the animal body of the active principles contained in Haffkine's plague prophylactic; and (b) experiment as to the inhibitory effect within the animal body, on the typhoid-fever bacillus, of antecedent, simultaneous, and subsequent introduction of the chemical products of those soil bacteria which Dr. Sidney Martin had found in the aggregate absolutely germicidal in the laboratory to the particulate cause of enteric fever. *Results*: (a) "Report by Dr. Klein on the Nature of Haffkine's Plague Prophylactic." (Animal experiment on rats, guinea-pigs, and rabbits.) (b) Statement by Dr. Martin that whereas alcoholic extract of the mixed chemical products in culture of soil bacteria were found by him inhibitive of the growth of the typhoid bacillus, a prolonged attempt by him to separate out the germicidal elements of such products had failed. He had been unable to obtain a relatively pure product having ability to inhibit the growth of *B. typhosus*. (Animal experiment, nil.)

Research for Investigatory Purposes

Work under this heading had to do with investigations of less immediate promise of useful application than the foregoing, but which nevertheless might yield results of high interest, and at the same time throw

light on problems maturing for the Board in the future. The researches in question comprised : (a) Resumption by Dr. Sidney Martin of his study of the chemical pathology of infectious disease, with preference in the first instance for diarrhœa of one and another class in reference to its microbic cause. *Result*: "Report by Dr. Martin on the Chemical Products of Diarrhœa-producing Bacteria." (Animal experiment on guinea-pigs and rabbits.) (b) Continuation by Dr. Houston of work on which he had been engaged previously. (1) Investigation of the fate of sewage microbes in soil. (a) The soil experiments to be, if practicable, with a sandy virgin soil free comparatively from spores of *B. enteritidis sporogenes*. (b) The soil to be tested before, during, and after inoculation with sewage, as to (i) total bacteria; (ii) spores of aërobic bacteria; (iii) spores of *B. enteritidis*; (iv) presence of streptococcus; (v) presence of *B. coli* and of *coli*-like microbes; (vi) relative amounts of gas-forming and indol-forming bacteria. (2) Further investigation of Chichester well waters. Chemically as to (i) free and albuminoid ammonia; (ii) chlorin; (iii) oxygen absorbed from permanganate. Bacterioscopically as to (i) *B. coli* and *coli*-like microbes; (ii) streptococcus; (iii) *B. enteritidis sporogenes*. (Special attention to be given to *coli*-like microbes, and *B. typhosus* to be diligently sought for.) *Result*: "Report by Dr. Houston on Inoculation of Soil with Sewage"; and "Report by Dr. Houston on Chemical and Bacteriological Examination of Chichester Well Water." (Animal experiment, nil.)

Sir William Power added that in 1901-2 there had been a threatening of plague; and the immediate testing of all suspected plague-material had been necessary not only in the discharge of international obligations, but in the interests of public health and of commerce. "The tests of suspected plague-material employed under the Board involve in almost every instance intra-peritoneal injection of

guinea-pigs; and in this way, during the year, a dozen or two of these animals were made the subject of experiment by Dr. Klein. Suspected plague occurrences in England and Wales in 1901 numbered forty-six, all but thirteen of them being instances of ship-borne disease."

The following questions were put and answered :

Q. Have you made many recommendations of prophylactics or remedies in the case of disease?—*A.* Yes, we have recommended the diphtheria antitoxin, and provided the diphtheria antitoxin. We have made some cholera preventives, Haffkine's method; and again we have done the same in regard to plague. We have prepared material.

Q. Taking those cases, would you recommend them?—*A.* When we have proved them.

Q. To your satisfaction and to that of your assistants, I suppose?—*A.* Yes, quite. It is essential that we should not recommend or issue anything unless we have satisfied ourselves as to its value and efficacy, and as to its involving no particular danger.

Q. In order to do that, are experiments on animals necessary?—*A.* Almost always.

Q. In all these cases that you have spoken of?—*A.* In all these cases I have spoken of, it has been essential to use the body of the lower animal in order to gain our knowledge.

Q. In the way you have described, by inoculation?—*A.* Yes, in the way I have described.

Q. Is it part of your duty to watch, or have you the opportunity of watching the effect of these remedies after you have recommended them, to see how far they are successful or unsuccessful?—*A.* Certainly; we get reports from the local officers, in the districts in which they have been employed, as to their efficacy in controlling the disease.

Q. Take those you mentioned; one you mentioned was Haffkine's fluid. You had satisfied yourselves, I presume, when you sent that recommendation out, that that was a valuable prophylactic?—*A.* We tested it very carefully on the rat before we issued it.

Q. And with many experiments?—*A.* A good many experiments. We did some by ourselves, and some in conjunction with Haffkine himself, who happened to be over from Bombay at the time.

Q. And all of them on the same animal?—*A.* They were all on the rat, except that some of the workers offered themselves for the test of experiment, wishing to get such protection as might be had by it; but those were the only human subjects in this country that to my knowledge really were operated on before this preventive was ready for issue.

Q. Do the same answers apply to the prophylactics against diphtheria?—*A.* Yes, the same was done with diphtheria.

Q. Were you satisfied with the success of that as a prophylactic?—*A.* Of the diphtheria prophylactic we, perhaps, have not so much evidence at present as to its value, at any rate not commensurate with its value as a curative agent. It is of very great value in that sense; but it has been used as a preventive not so much as one would have desired, partly because it has not been made generally available. There is a question as to paying for it by the local authorities.

It was no part of the duty of the Local Government Board to try to discover remedies: it was their endeavour to find out the effects of remedies which had been put upon the market, so that those which appeared to have value should be used, and not those which had none. "But, at the same time, we should perhaps make some original

investigation towards getting a preventive. For instance, I think it is quite possible that the Board might engage in some such investigations in regard of cerebro-spinal fever, which is now apprehended as coming upon us. Probably there will be a great demand, before long, for a prophylactic, and for remedies for the disease; and it is quite possible that a good many very doubtful things may be put upon the market, and we probably shall have to test them; and while we are doing that, we might make some investigations of our own to see whether we could make anything of any value as a remedy."

The witness was asked, "At the present time, has a port sanitary authority any means of diagnosis so sure as that of experimentation on animals, with regard, say, to plague?" He answered, "No, nor with regard to cholera." It was essential that the diagnosis should be not only accurate, but prompt; because, in the meantime, a ship which had brought the disease might be hung up, and trade delayed, and commerce damaged.

Asked whether the experiments on animals which came under his Department were practically confined to inoculations, he said that they were. "If there is pain or discomfort it will be in the after-effects; but in the great majority of instances, similar after-effects would not be notably painful in the human being; they might cause some discomfort, but not more than that."

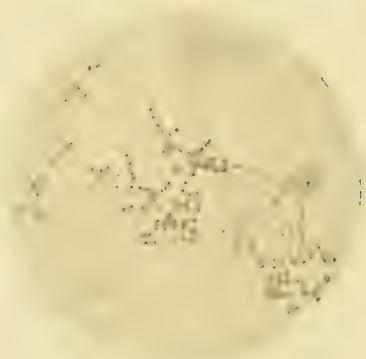
SIR R. DOUGLAS POWELL, Bt., K.C.V.O.,
AND DR. FREDERICK TAYLOR, M.D., F.R.C.P., *March 5, 1907*

Sir Richard Douglas Powell, President of the Royal College of Physicians, Physician Extraordinary to King Edward VII., Consulting Physician to the Middlesex Hospital, the Brompton Hospital, and the Ventnor Hos-

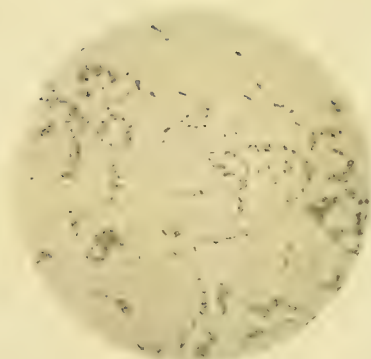
pital, and Dr. Frederick Taylor, Senior Physician to Guy's Hospital, and sometime Censor of the Royal College of Physicians, gave evidence as representing the Royal College of Physicians. They handed in a memorandum of their general views. "Those of us," said Dr. Taylor, "who are constantly thinking over these problems of disease, how it is to be recognised, and how to be treated, believe that it is absolutely necessary that constant research and constant experimentation and observation should be made in order to get at the problems of disease, which are extremely deep and extremely complex, and, one might say, get deeper and deeper almost every day. And, while we are not making those observations and researches ourselves—I mean to say so far as experimentation on animals is concerned—we feel very deeply the absolute necessity of such observations being made, in order to provide us with the means of dealing with disease in an efficient way. Much, of course, can be learned by observation: that is what we are struggling with every day in our clinical observations in hospitals and amongst our private patients; but it must be helped by experimentation. And the same is true with regard to drugs: if new drugs are introduced, they must be tested. Either we must blindly experiment on ourselves and our patients, or else, as it seems to us more wisely, we must experiment on animals, to find out what the effect of a drug is, before we can use it on the human body. Perhaps the most striking instance of the necessity for that kind of experiment is the fact of the use of nitro-glycerine. Nitro-glycerine, which was first, of course, known as an explosive, does not suggest itself, even to a medical man, as the most likely thing to do much good in the human body; but, by experimentation with nitro-glycerine in extremely small quantities, we know that it has certain properties, one, for instance, being that it

causes dilatation of the blood-vessels; and it is largely used at the present time for that purpose, but, of course, in extremely minute doses, the dose given at first being one hundredth part of a drop. I think it must be obvious to every one, that our ascertaining a fact of that kind must almost necessarily have been derived from experimentation upon something other than our patients: and animals, of course, had to be employed for that purpose." Dr. Taylor went on to speak of experiments for the study of the infective diseases. "Animal experimentation is practically the basis of bacteriology, and there does not seem to be any possibility that that study can be continued without experimentation being carried on in that way. And to say what bacteriology has done, or what bacteriology is doing, or what bacteriology is hoping to do in the future, over many years yet maybe, is simply to repeat what we have said in our memorandum. Vaccination with respect to smallpox is only, after all, the pioneer of bacteriology. Diphtheria, tetanus, tubercle, typhoid, Malta fever, snake poison, yellow fever, the infections of pneumococcus, streptococcus, staphylococcus (these last two being forms of septicæmia) have all been influenced materially by bacteriology, and there are many hopes indeed that they will continue to be influenced by bacteriology and its necessary attendant, vivisection of some kind—that is, on either a human being or animal. And, of course, to attempt to stop that process of observation does not seem a very profitable effort to make."

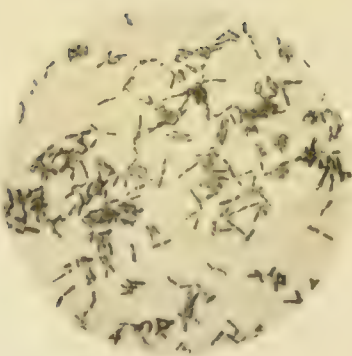
Dr. Taylor went on to speak of the great value of diphtheria antitoxin. "Especially in that form of laryngeal diphtheria which attacks the larynx, so that patients run the risk of being strangled in the early stages of the disease, the chances of recovery are much greater since the antitoxin treatment has been used. More recover without the



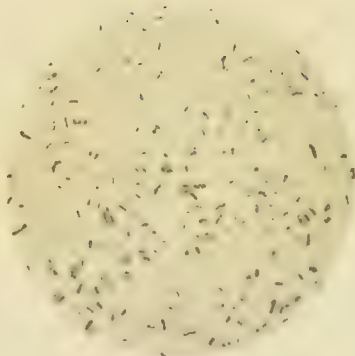
STREPTOCÖCCUS.



BACILLUS MALLEI.



BACILLUS DIPHTHERIA.



BACILLUS PESTIS.

necessity of having the operation of tracheotomy; and those who have tracheotomy recover in a greater number than they used to do. Before that time, the fatality in those cases was so great that there were some physicians and surgeons who felt it was almost useless to operate by tracheotomy, because the certainty of death seemed so great at that time. Certainly our position, with regard to that, is very different at the present time."

The following questions were put to Sir R. Douglas Powell:

Q. You are President of the Royal College of Physicians?—*A.* Yes.

Q. And you and Dr. Taylor appear on behalf of the College?—*A.* Yes.

Q. May I ask the membership of the Royal College of Physicians—what body of professional opinion the College of Physicians represents?—*A.* It consists of some 350 Fellows, and a larger number of Members, and, I think, some 10,000 or 11,000 Licentiates.

Q. Do you come here to-day as representing the Fellows, or as representing the Council, or whom?—*A.* I have been invited to come here as representing the College of Physicians, and, I presume, through the College of Physicians, more or less the profession of medicine.

Q. I wanted to know how far your opinion is backed up by the profession generally.—*A.* I only speak as a physician, and I believe I represent the general opinion of the profession: I have not canvassed them.

Q. Your views, as put in this paper, have not been formally discussed by the Council?—*A.* No.

Q. So far as you know, the opinions which you have expressed to-day are the opinions of the leading members of the profession?—*A.* Yes.

Q. Can you give us the name of any leading member of

the College of Physicians who holds a different opinion, and thinks that animal experimentation either is wrong or leads to erroneous results?—*A.* No, I really cannot. I do not know of any one.

Q. Among your own Council, is the opinion practically unanimous, then?—*A.* Yes.

Q. And your deliberate opinion is, that experiments on animals are necessary to the successful carrying out of the practical profession of a physician?—*A.* The carrying out of the profession of a physician is largely based upon the results of experiments on animals.

Q. And the practice of medicine would be injured if experiments on animals were stopped, you would say?—*A.* Unquestionably.

Later, the following questions were put and answered :

Q. (*To Sir R. Douglas Powell*). I should like to ask whether either of you gentlemen have ever met anybody, who has been in the habit of constantly treating diphtheria, who does not believe in the value of antitoxin in the treatment of diphtheria?—*A.* I should say not, certainly.

Q. You have never met with such a person?—*A.* No.

Q. (*To Dr. Taylor*). Have you ever met with any one, who has been in the constant habit of treating and seeing treated cases of diphtheria with the antitoxin, who has not been perfectly certain of the value of it?—*A.* I have never met anybody holding any other view. I should say that all whom I have met with and talked with about diphtheria have been unanimous in seeing the desirability of using the antitoxin whenever the first signs of diphtheria were recognised.

Q. (*To Sir R. Douglas Powell*). With regard to the use of tuberculin, is it used to any extent now, in the treatment of human beings?—*A.* It is used in a certain

number of cases. It is used in a different way from that in which it was used in Koch's time: it is used with more safeguards. When tuberculin is used now, the blood is examined from time to time, in order to ascertain whether the tuberculin has resulted in raising what is called the index of resistance to tubercle: that is the object of giving tuberculin. And tuberculin, at the present time, is not pushed to those large doses, and is not given in those repeated doses, which tend to lower the index of resistance rather than to raise it. That is the difference between the two treatments. The actual value of tuberculin as a treatment of consumption is at the present time still *sub judice*. My own belief is that it is valuable in certain cases. I think I have seen it valuable in certain cases.

Q. And in such cases as you have spoken of, where it has been given in comparatively small doses, so as to increase the index, have you known of cases where it has done decided good?—A. Yes, I have.

Having given his evidence, Dr. Taylor added, "May I make one remark with regard to the last paragraph in our Memorandum: 'We would, in conclusion, say, on the part of the whole medical profession, that we have no less regard and sympathy for suffering animals than others, nor any less urgent desire to spare them, so far as is compatible with the larger claims of humanity.' I do not know that the public always recognises what we, as medical men, know and feel with regard to human sufferings. We see patients day after day, patients going on month after month, suffering from diseases which we are trying to alleviate or to prevent their termination. That seems to me to involve a very large element of sympathy that we have a right to feel, I think, for humanity, as others feel for dogs and horses; and I do think that from that point of view the subject has not always been suffi-

ciently put before the public when this matter has been discussed. Much has been made, for instance, of the subject of hydrophobia. There are not many people in England who have seen hydrophobia. I saw two cases some years ago; and anything more horrible one can scarcely conceive, than to see a child of ten years of age, as I did, dying as it was for two or three days of that disease. Any one, I think, who had any sort of feeling whatever, would feel that a good many animals might have been sacrificed in the interest of saving that child's life, and preventing the distress to her parents and all others connected with her. And so I feel with regard to the whole matter. It is said that there are hundreds of dogs experimented upon; but in the whole country—the whole world—there are thousands of people suffering. We see it every day of our lives; and those who have lived a few years, and have had a large experience of that kind of suffering, I think, have a right to desire to improve their means of alleviating those sufferings; and if it can only be done by means of vivisection, then it is a fair question for consideration whether the practice should not still be maintained."

SIR T. LAUDER BRUNTON, M.D., F.R.C.P., F.R.S.,

April 24, 1907

Sir T. Lauder Brunton, Consulting Physician to St. Bartholomew's Hospital, was asked about the alleged "baking to death" of cats and rabbits, by Claude Bernard and other physiologists. He explained that the animals were only put in a box or tin vessel under conditions like those of a Turkish bath: they were heavily narcotised with chloral or opium, and wrapped in wool, so that their temperature rose, as the temperature of fever patients rises. The animal died of this excessive rise of its temperature.

When the animal was completely senseless, a cannula was introduced into the trachea, and an ordinary sewing-needle was put between the ribs to record the action of the heart. The animal did not make the slightest movement: the experiment was absolutely painless throughout. He described some experiments of the same kind, made on pigeons, to test the value of aconite in fever. "This box was simply a metal box with a glass top, and surrounding the metal box was a second case, in which you could put either hot or cold water, so as to raise the temperature of the box to such a degree as we liked. We padded the inside with brown paper, so as to prevent any contact of the animal with the metal itself, because we did not want any local chill or any local burning to vitiate the experiment. All we wanted to do was to find out the condition of fever such as would occur in a patient in bed. We raised the temperature of the animals by raising the temperature of the box, in some cases to 106° Fahr.; but this can hardly be described as baking alive, because I have found by inquiry at the Jermyn Street Bath that, whereas our highest temperature was 106° Fahr., the lowest temperature of the Jermyn Street Bath is 120° Fahr., their highest temperature running up to 230° ."

Sir Lauder Brunton was asked whether he thought the Act was hindering science, and he said that, to a certain extent, he did. The following questions were then put and answered:

Q. I presume you would be very much opposed to any further restrictions upon experiments upon living animals?
—*A.* Surely. Perhaps I might be allowed to give one illustration of how the Act has interfered with work which might have been beneficial. About 1871, with Sir Joseph Fayrer, in making experiments upon snake-

venom, we found out that there were certain substances which would destroy snake-venom absolutely, but we had not succeeded in obtaining immunity, or saving life after a big dose had been injected. We could not administer the antidote in such a way as to prevent the action of the poison when once introduced. Then came the anti-vivisection laws, and we could not continue our experiments at all at home. The consequence of that was, that they lapsed for a number of years : and they were only taken up again about two years ago by Major Rogers. He was able to take them up again because Professor Waller had invented a method of giving chloroform continuously for thirty hours or more, so that he was able to give the poison and then keep the animal for many hours under the anæsthetic ; and then he could try the effect of the antidote. I had invented a small lancet, which I thought might be useful, but it had not practically been tried until Major Rogers got this chance of doing it, when he found that it answered quite well. He has since been using it in India, and has, I think, now got either seventeen or twenty cases of snake-bite in which the persons would almost assuredly have died, and who have been saved by the use of the instrument. I have one here ; it is made in two forms (*exhibiting the same*). It consists of a small lancet, in the handle of which is some permanganate of potash. In using that, the punctured wound made by the snake's fang is simply converted into a slit, and permanganate of potash is rubbed in, after being wetted with saliva.

Q. You claim that your little lancet has been discovered by the assistance of experiments on living animals ?—

A. It was made in consequence of a number of experiments that I made with Sir Joseph Fayrer.

Q. The cure is by permanganate of potash ?—A. Yes.

Q. All obtained by experiments on living animals?—
A. Yes; experiments which were made quite thirty years ago.

Sir Lauder Brunton was then asked about the discovery of amyl nitrite for the treatment of angina pectoris. Had not Sir Benjamin Ward Richardson made this discovery by testing the drug on himself, not on animals? Sir Lauder Brunton answered that Richardson's chief account of amyl nitrite, in the Report of the British Association for 1864, dealt much more with experiments on animals than with the experiment on himself—"He did suspect, and correctly, that the excitement of the heart was probably due to the effect of nitrite of amyl in dilating the capillaries and lessening the resistance; but he did not pursue that any further. He went on, in his report of 1865 and later, to discuss the question of the action of other compounds of amyl, and left the nitrite, which was the most important, alone: so that the question of how it acted remained unsolved. It was then taken up by Rutherford and Gamgee, who made experiments upon animals with an ordinary manometer, and found that the blood-pressure was greatly reduced by nitrite of amyl. At that time I was working a good deal in the laboratory with Gamgee, and knew all about the experiments. I had made none on nitrite of amyl myself, but I was very susceptible to nitrite of amyl, and Gamgee very often tested it upon me, taking traces of my pulse; and then I saw the extraordinary depression in the tension of the pulse which was produced by nitrite of amyl. At that time I was house-physician in a hospital in Edinburgh, and there was one poor man in the wards suffering dreadfully from angina pectoris; he used to have an attack every night, and for two hours the unfortunate man would sit on the edge of his bed, and could not move forward, backward, or

to one side, with his face pale and the sweat pouring off it, in perfect agony. I used to sit by him and try to do what I could to ease his pain. I gave him chloral, ether, alcohol, everything I could think of—it was all no use. I took tracings of his pulse, to see what was happening, and I found that his pulse, during the attack, became very tense. I tried everything that I could think of, and one day he said, ‘Well, you have been very good, but it is no use; let me go.’ I said, ‘Well, I will try one thing more, and if it does not do you any good you shall go out to-morrow.’ I went straight up to Gamgee and asked him for some nitrite of amyl, because I thought, if nitrite of amyl lowers the tension so much, and this poor man’s tension rises during the attack, if I gave amyl it ought to ease him. I brought it back and gave it, and it was perfectly successful. But for the experiments, I should never have known the action of nitrite of amyl; nobody would have done so.”

As further instances of methods of treatment discovered or improved by the help of experiments on animals, Sir Lauder Brunton mentioned the use of new soporific drugs, the diagnosis and treatment in cases of heart disease, the recognition of the state of the blood-pressure, the advance of abdominal surgery, the treatment of dropsy, the treatment of myxœdema and cretinism, and the hypodermic method of giving drugs. Asked about this last instance, he answered: “The way it came about was this. Sir Robert Christison was a student of Orfila’s in Paris; and Orfila made a very complete series of observations on the action of all kinds of drugs upon animals, which he recorded in his ‘Toxicology.’ Sir Robert Christison was a pupil of Orfila’s, and used to talk a good deal about Orfila and his experiments; he talked about them to old Dr. Alexander Wood, and Alexander Wood somehow or another

thought that, instead of using the intravenous injection of which Orfila was so fond, if he injected into the subcutaneous tissue he would get the same result; and he tried it, and it answered: and that is how it came about. I do not know whether Dr. Alexander Wood published definitely this, but what I am giving you now is the result of what I used to hear when I was a student, when I knew Christison, and slightly knew Wood."

Asked about a statement in his *précis* that the nature of consumption had been discovered through experiments on animals, he answered: "We now know that phthisis depends upon an invasion of the lung by a certain bacillus, the tubercle bacillus; that in many cases, however, the infection is not a pure infection, but a mixed infection; there are various other microbes associated with the tubercle bacillus, which aid its invasion of the body. But if we could simply prevail upon all people to recognise the infective nature of phthisis, to prevent spitting in all public places, to take proper precautions against the conveyance of the tubercle bacillus from one individual to another, in thirty years phthisis would be just as rare in this country as leprosy is now. I believe that all tubercular disease could be stamped out entirely in this country in thirty years. The chief objection to it is the expense; because, in order to stamp out tuberculous disease, you would probably require to slaughter a large number of cattle."

The following questions were put and answered:

Q. With regard to some experiments which have been mentioned in this room by witnesses on both sides—experiments upon digestion and upon the passage of food down the intestines—it has been stated here that all that was of value was learned by Dr. Beaumont from the examination of the stomach of Alexis St. Martin. How far do you think that is true?—A. That is not correct, of course,

Dr. Beaumont's experiments were very valuable indeed, because it was a case of experiment upon man. The side of poor Alexis St. Martin was blown away by a charge of duckshot, so that they could see inside, and Dr. Beaumont made very good use of his opportunities; but that only went a very short way. It showed that the gastric juice had the power of digestion; it showed that when Alexis St. Martin drank too much spirits his stomach got very much out of order; and that was about the end of it.¹ Dr. Beaumont also found that if he stirred up the stomach very much with a glass rod, or something of that sort, Alexis St. Martin might feel a little sick; but the whole of the rest of the practical introduction of such things as pepsin, pancreatin, and all the adjuncts to digestion, have resulted from the experiments upon animals made afterwards by Blondlot, by Bernard, Bedder, and Schmidt, and others, and the most recent have been made by Pawlow.

Q. (Sir W. Church). In your opinion, is gastric fistula in an animal, or an intestinal fistula in an animal, a condition in which it suffers great pain?—*A.* Certainly not.

Q. You say that confidently?—*A.* I say that confidently.

Q. Why do you speak so confidently?—*A.* Because, years before the Act, I made a gastric fistula in an animal, in a dog, which never showed the slightest sign of pain; and when I wanted to examine inside the stomach, it showed great delight—just like a dog that has been sitting about the house, and wants to run out for a walk. When it saw

¹ Dr. Beaumont, in his account of this case, especially says that it did not lead him to any great discovery. "I make no claim to originality in my opinions, as it respects the existence and operation of the gastric juice. My experiments confirm the doctrines (with some modifications) taught by Spallanzani and many of the most enlightened physiological writers" ("Experiments and Observations on the Gastric Juice, and the Physiology of Digestion," by William Beaumont, M.D. Edinburgh, 1838).

that I was going to look into its stomach, it frisked about in the same way as if I was going to take it out for a walk.

Q. (Col. Lockwood). How did it know what you were going to do?—*A.* Because I did it day after day. I made the fistula, and the fistula healed up. Then I had a cannula in the stomach, and corked it up so that I could look in.

Q. I confess that I am rather aghast. If I understand you rightly—I do not want to do you injustice—the dog showed great delight when you were about to perform an operation on its stomach?—*A.* No. After the operation had been done, and the thing was healed up, when I wanted to examine it, it seemed delighted.

Q. Because it afforded it relief, I suppose?—*A.* No; it was because it liked to be made much of, and petted and shown round.

Q. (Sir M. Chalmers). It liked to be taken notice of?—*A.* Yes.

Q. And there was no pain?—*A.* There was no pain.

Q. (Sir W. Church). As a matter of fact, in men, is the opening of the stomach a very painful operation?—*A.* No.

Q. In fact, the actual opening of the stomach is usually done without any anæsthetic at all being administered. The attachment of the stomach to the wall is done under anæsthetics?—*A.* Yes.

Q. And after the stomach has been opened, the patient seldom or never experiences pain in the passage of the tube into it for food?—*A.* That is so.

Later, the following questions were put and answered:

Q. I suppose that when a man faints at seeing an operation it is really because he is sorry for himself, and not because he is sorry for the patient?—*A.* Yes.

Q. And when he gets used to it, it ceases to affect his own nerves, but it does not affect his feeling for the patient?—*A.* That is so: that is very truly put.

Q. You are a Fellow of the Royal Society as well as a Fellow of the Royal College of Physicians?—*A.* I am.

Q. How long have you been a Fellow of the Royal College of Physicians?—*A.* About thirty years; and I have been a Fellow of the Royal Society, I think, for thirty-three years.

Q. In this inquiry we are anxious to get evidence from both sides. Can you give us the name of any Fellow of the College who could give us evidence against the view that you have been taking to-day?—*A.* No, I do not know of any. Of course, there might be such.

Q. But you do not know any one whom you could suggest to us?—*A.* No.

Q. Now I want to go more into general questions. There is a great deal of feeling, naturally, about dogs. If experiments on dogs were prohibited, would medical science suffer?—*A.* Yes.

Q. You think they are essential?—*A.* It is essential that dogs should be used.

Q. Would you give us your reasons?—*A.* Because other animals are too small. For many of the experiments guinea-pigs, rabbits, and cats are too small; and other animals of the same size, such as sheep, goats, and pigs, are too expensive.

Q. Is there any importance in the fact that the dog is carnivorous?—*A.* I think there is. I think that in experiments, for example, on the composition of chyle, or digestive experiments, they are very important.

Finally, Sir Lauder Brunton was asked to make some comments on certain statements made by Sir James Thornton against experiments on animals. He was questioned over these statements, point by point. He said that his opinion was, definitely, that but for experiments on animals abdominal surgery would not now exist. He said

that the action of calabar bean on the eye was discovered first of all by Fraser in his experiments upon animals. He was quite willing to grant that we did not know anything like the whole pathology of diabetes yet; but the experiments of Claude Bernard and others had taught us a great deal about the nature of the disease. He said that our knowledge of the processes of digestion was all founded on experiments on animals. He said that his own experience was that diphtheria antitoxin was an extraordinarily valuable thing; that it practically robbed diphtheria of much of its terror. Asked about Sir James Thornton's statement that experiments with drugs on animals were quite useless, he answered that it was made in pure absolute ignorance. Asked about Sir James Thornton's instances of substances poisonous to man, but not poisonous to animals, he answered: "A number of the instances are quite correct. The general conclusion is quite erroneous." Asked about Sir James Thornton's statements as to the preventive treatment of rabies, he answered as follows:

Q. Have you any practical experience of the Pasteur treatment?—*A.* Yes; I was a member of the Commission sent across to investigate the utility of Pasteur's treatment. I went over, perfectly convinced that Pasteur was wrong.

Q. What was the date of that Commission?—*A.* About 1884 or 1885.¹ But I came away, perfectly convinced that Pasteur was right. And there is no doubt whatever, I think, in the mind of any unprejudiced person, that

¹ The exact date of the letter appointing this Commission is April 12, 1886. The members of the Commission were Sir James Paget, Sir Lauder Brunton, Sir George Fleming, Lord Lister, Sir Richard Quain, Sir Henry Roscoe, Sir John Burdon Sanderson, and Sir Victor Horsley (Secretary). They presented their Report in June 1887.

Pasteur's treatment has produced a complete revolution in the treatment of hydrophobia. Hydrophobia was a most frightful disease. Just before I went over I was a member of a Committee of the British Medical Association to investigate hydrophobia; and I went throughout all the country examining cases, and the cases I saw were simply heartbreaking. As the result of it I got inoculated, and had to burn it out.

Q. Your own finger got inoculated?—*A.* Yes, unfortunately, with rabid poison when making a post-mortem examination.

Q. What treatment did you pursue yourself?—*A.* Caustic potash; there was nothing else. I took a stick of caustic potash and rubbed it in, and scraped it out, and rubbed it in again.

Q. Did you go through Pasteur's treatment?—*A.* No, it was before Pasteur's treatment; that was the only treatment available at the time, but it does not improve the fingers.

Q. You satisfied yourself on that Commission that Pasteur's treatment saves life?—*A.* Absolutely.

MAJOR LEONARD ROGERS, I.M.S., *May 15, 1907*

Major Leonard Rogers, M.D., F.R.C.P., Professor of Pathology at the Medical College, Calcutta, stated that our present knowledge of the physiological action of snake-poisons, and of the antidotes for snake-poisons, was founded entirely on the results of experiments on animals. He referred to the early experiments of Sir Joseph Fayrer and Sir Lauder Brunton on the cobra venom, and to the experiments by Martin and others on the viperine venoms. "The viperine class of poisons have a totally different action from that of the cobra class, *i.e.* the poisonous

colubrines. It was first shown by Professor C. J. Martin, now of the Lister Institute, that one of the snakes in Australia caused intravascular clotting, and he suggested that this was probably the physiological action of the viperine class; and it was afterwards shown by Lamb, in Bombay, that Russell's viper does cause death in that way. Four years ago, I investigated this subject at the Physiological Laboratory at the London University, with Dr. Waller; and I tested vipers, both Russell's viper of India, the rattlesnake of America, and the African puff-adder; and I found that in addition to this intravascular clotting you also got paralysis of the vaso-motor centre, and that that is the most essential cause of death in the case of viperine snake-poisons. I also found that it could be counteracted, to a considerable extent, by the action of drugs, especially adrenalin, which contract the vessels, and so counteract the fall of blood-pressure. I published the experiments in a paper read before the Royal Society, and illustrated my point with tracings. So that our knowledge of the viperine poison's physiological action is also due entirely to experiments on animals.

"The next point in the action of snake-poisons is the question of the action of antivenins, first discovered by Calmette of Lille, and independently by Sir Thomas Fraser. They found, as the result of repeated inoculation of cobra-venom into animals such as horses over a period of many months, taking usually eighteen months, that they were able to produce a powerful serum or antidote in the blood of the infected horses, which has a definite specific action against the cobra-venom; so that after injection of the cobra-venom into an animal it is possible to save that animal's life, by the injection into a different part of the body of a proper dose of the antivenin. In some experiments which I have done on this question, too, I found that the results

are very much better if the antidote is injected intravenously. If the anti-cobra serum is injected intravenously, it has exactly the same action upon the system of the animal that it has if mixed with cobra-venom before injection; but if it is injected subcutaneously, it is absorbed more slowly, and does not act so efficiently. There is one difficulty with regard to the practical application of this treatment, which is, that the serum made from cobra-venom does not act at all against the venoms of the viperine class, which have a different physiological action; and further, the antivenin made from cobra-venom only has a slight action against other colubrine poisons, such as the krait or the hamadryad. Therefore, although the antivenin has a decided protective influence, it is not practically of very great use. It is not on hand, for one condition: you seldom have both the antivenin and the syringe on the spot; and even if you have, it really requires much larger doses than are usually given, and a very skilled operator—no village man could do it. There have been a number of cases reported, where it has apparently had a good effect. The best example was the case of Captain Lamb. He was investigating snake-poison in Bombay, and he was bitten by a cobra in the thumb, one fang entering. They were testing antivenin at the time, and the antivenin which they had they found was not strong enough to be of much use, so that they had to send for some more. It was an hour before they got an efficient serum, and by that time Captain Lamb was beginning to show symptoms of drowsiness and early symptoms of cobra-poisoning. After an injection of the antivenin he recovered perfectly well, with some local sloughing, which you always get after such poisoning. That is a definite case where antivenin appears to have saved life."

Asked when he first took up the subject, Major Rogers answered: "I was working first at the sea-snake poisons in Calcutta in 1901, when I found that they were very poisonous, and that their venom had the same action as the cobra-venom. Then, when I was in London in 1903, I was working at the physiological action of the sea-snakes and the viperine poisons which I have mentioned; and then Sir Lauder Brunton suggested to me the carrying out of experiments to test the value of the method he had previously suggested, of a local application of permanganate of potash by incising the seat of the bite, or the injection of venom, and rubbing in the crystals. He requested me to continue the experiments. He had previously published the suggestion of making the snake-lancet, and it had actually been made; but, as it had not been thoroughly tested and proved, it had not come into practice: and he asked me to take the matter up at that point. The first thing I did was to test the action of permanganate of potash on a number of different venoms, as it was previously only known that it destroyed cobra-venom. I tested it against all the Indian colubrine poisons, the krait venom, the hamadryad and the banded krait, and the sea-snakes, and also against Russell's viper of India, the rattlesnake of America, and the African puff-adder; and I found that mixing a strong solution of permanganate of potash with any of these venoms would destroy the venoms in almost the same weight as the permanganate of potash; so that it has a very powerful action. Sir Lauder Brunton told me that it was owing to the passing of the Antivivisection Act that his experiments were stopped; he was unable to go on. At that time he was trying injections of a solution of permanganate of potash as a form of treatment; but that apparently is not an efficient method of using it. What he told me was, that his experiments were

stopped by the passing of the Act. Then, next, I started carrying out experiments on animals as nearly as possible under conditions in which the permanganate of potash would be used in actual practice. I used rabbits and cats. The animals were all chloroformed; then a measured dose of cobra-venom was given, in accordance with the weight of the animal; so many lethal doses, or, rather, so many times a fatal dose. The injections were made a little above the foot; ordinarily, snake-bites are on the hand or foot, as a rule. After the measured time had elapsed, varying with the different experiments, a ligature was applied above the seat of the injection, in order to stop the circulation. An incision was then made at the seat of the injection, with a small lancet—the actual lancet which is now being sold in India—and at the seat of the injection, within thirty seconds, I found that there was always a small blood-stained effusion; which is of practical importance, at any rate, as it shows the exact seat of the venom. A few permanganate crystals were then poured into the wound and rubbed in, until the wound was completely blackened. This took one or two minutes. The ligature was then immediately removed, on the supposition that the poison had been destroyed by the application of the permanganate of potash. The cats were then allowed to come round—I had reason, of course, for doing that—and kept from five days to a week. They appeared to suffer no pain; in fact, the only difficulty I had with them was, that they all wanted to be petted at the same time if I went to see them. As the result of the experiments I found that, after the injection of ten times a fatal dose of cobra-venom, I could save the animal by operating thirty seconds afterwards. That is about the time it takes to apply the ligature if a person is bitten in a limb. I lost one animal after the injection of five times a fatal dose—it was the first animal

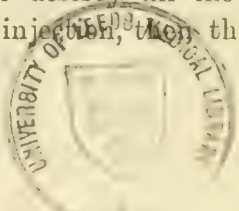
done—the application not having been efficiently carried out as in the latter case; but its life was very greatly prolonged, as compared with the control animal injected with the same dose without the treatment. Then, I used decreasing doses of venom at longer intervals; and I have now worked up to getting a recovery in the case of a cat not treated until half an hour after the injection of a double lethal dose. Similarly, I tested with Russell's viper, as a type of the viperine class; and there I have got recoveries up to five times a fatal dose, which is more than any viper I know of can inject, judging from the amounts I have obtained from these snakes, in proportion to the weight of a man. That is the experimental basis of the method.

“With the printed instructions giving directions for the use of the lancet, supplied with each lancet, I ask to have reports sent to me; and I have received a number of reports during the last three years, which I found it necessary to classify in two different classes. In judging of the action of any remedy in the case of snake-bite, there are a great many fallacies to be avoided. In the first place, the patient may not have been bitten by a poisonous snake; and in the second place, he may not have received a fatal dose. Consequently it is owing to this, that so many remedies have been brought forward as cures for snake-bite, which have been found experimentally not to be efficient. In order to try and get absolutely accurate figures, I have taken, in my first class, only the cases in which the snake has been killed and identified as a poisonous one, and in which there were definite fang-marks where undoubtedly the patient had been bitten by a poisonous snake. The snake has been identified as a poisonous one by the hospital assistant; in some cases, by qualified medical men. Many years ago, as the result of Fayrer's work, the Government

of India provided coloured plates of different poisonous snakes to all the dispensaries in India ; so that these men are trained to recognise these snakes. The snake is killed and brought to the dispensary, and identified by the assistant surgeon, who has been trained, and has these coloured plates at his disposal.

“ Among these fourteen cases where the snake was killed and identified, nine were cobras, three Russell’s vipers, and two kraits. Among these fourteen cases there was one death : and this was a woman who was brought to the hospital eleven hours after the infliction of the bite, when she was quite moribund. This treatment was undoubtedly established as the result of experimental work on animals. When the method was first brought out, the Government of India declined to adopt it on Sir Joseph Fayrer’s recommendation ; but since these results have been published, they are now widely distributing the lancet in every village in several provinces—having been convinced of the utility of the method—at the public expense. It is being sold, as well as supplied. Over 7,000 lancets are sold by one firm in Calcutta. Every European going shooting, and that sort of thing, practically invariably takes one.”

Asked whether he instituted any comparison between Calmette’s antivenin and the permanganate treatment, Major Rogers answered : “ The two should be combined. The permanganate is practically a method which applies to every kind of snake. Calmette’s method does not, because the antivenin which is made for cobra is only useful for cobra. An antivenin has been made for Russell’s viper recently, but it will only work for Russell’s viper. And, further, the amount of sera required is very large, larger than is often practicable. But if the permanganate treatment is being used to destroy all the venom remaining locally at the seat of injection, then the use of antivenin



becomes much more likely to save life, as it would have only to act against a much smaller amount of venom. So that if you happen to get a patient who has just absorbed a fatal dose, so that the permanganate treatment would be too late to save life,—by destroying all the venom remaining locally by the permanganate method you could then save that patient's life by a comparatively small dose of the antivenin." Major Rogers added that they were preparing, at the Pasteur Institute at Kasauli, a serum which should be efficient both against cobra and against Russell's viper. Asked further as to the Government distribution of Brunton's lancets, he said: "You have to take into account the conditions in India. If you take, say, Eastern Bengal, where so many snake-bites occur, and where almost every house is separate, with a large number of ricefields between, there is practically no such thing as a village. When you come to hundreds of thousands of square miles of country like that, you can see how difficult it is to have the remedy always on the spot. But the Government are doing now a great deal towards distributing it, and I expect that we shall get good results."

Major Rogers went on to say that he had found the permanganate very useful in preventing tetanus infection in wounds. "In Calcutta, tetanus is an exceedingly common disease, because the whole of the road dust is full of it. So I carried out some experiments, by putting a little dust from the Calcutta road under the skin of a rat, and I found that five out of six died of tetanus: but if I added a few crystals of permanganate of potash to the dust, then I only lost one animal, and that was not from tetanus. I then carried out a further series of experiments, by scarifying the skin, rubbing in the dust, and using strong permanganate of potash solutions at different intervals. And I found by that measure also, even up

to an hour afterwards, it was nearly always possible to prevent tetanus infection, although the control animals developed the disease. But permanganate of potash has also been used, at my suggestion, in cases of leopard bites. When persons are shooting in the wilds and there is no doctor near, permanganate crystals instilled into leopard bites, which are usually very fatal, in severe cases have prevented serious results from blood-poisoning. The serious results arise simply from the dirtiness of the leopard's teeth. I also suggested that it was worth trying in hydrophobia: and one of the medical officers in the south of India has used it in a number of cases of dogs which have been bitten by mad dogs; and a number of them have been saved apparently, and he is very enthusiastic about it. There is a considerable death-rate from tetanus at Calcutta at the present time. They are using the tetanus serum in Calcutta as a prophylactic. It is of very little use after symptoms have developed, but it is now used at the Medical College Hospital in all wounds that are thought to be likely to be infected with tetanus, and it has very materially reduced the number of cases of tetanus at the Medical College Hospital in the last few years."

NOTE.—For the preventive use of the serum, to protect horses against tetanus, see Mr. Stewart Stockman's evidence, Chapter VIII. See also Dr. Warbasse's account of the preventive use of the serum in the United States, in those cases of lacerated wound of the hand which occur after the Fourth of July celebrations. "One of the wounds most commonly followed by lockjaw is the blank-cartridge wound of the hand common on the glorious Fourth of July. The death-rate from these wounds is appalling. An active campaign has been conducted throughout the medical profession to reduce this mortality. All over the country,

surgeons and medical journals have advised the injection of tetanus antitoxin in every case of blank-cartridge wound. The American Medical Association has compiled statistics of Fourth of July fatalities for the past six years. Six years ago the Fourth of July tetanus cases numbered 416. Then physicians began a more general use of antitoxin in all cases of blank-cartridge and cannon-cracker wounds. As a result of this campaign of prophylaxis, by antitoxin injections, from 416 cases of tetanus in 1903, the number dropped to 105 cases in 1904, 104 cases in 1905, 89 cases in 1906, 73 cases in 1907, and 55 cases in 1908. This reduction in the number of tetanus cases took place while the number of accidents remained practically the same each year, and while the number of deaths from causes other than tetanus was steadily rising from 60 in 1903 to 108 in 1908. It is thus evident that the saving of at least 300 lives from tetanus has been accomplished each year through the prophylactic use of tetanus antitoxin in the cases of Fourth of July wounds alone. These represent but a small portion of the wounds, through the whole year, in which it is used." (The Conquest of Disease through Animal Experimentation. By J. P. Warbasse, M.D. Appleton & Co., New York and London, 1910.)

Major Rogers was then asked about his work for the Indian Government on rinderpest. He said that he was in charge of the Government laboratory all through 1899. "I may say that, at the end of my year's work, I made arrangements for turning out 100,000 doses of serum a year. I also inoculated a large number of animals myself in actual outbreaks. In two districts, I inoculated over 500 animals that were actually in the same sheds with other infected animals; and at the same time I had notes of all the other animals, similarly exposed to infection,

which were not inoculated because the villagers did not agree to it. These were all subsequently reinvestigated at the end of two months. And it was found that no one of the 500 animals which had been inoculated had developed rinderpest later than four days after the inoculation—that is to say, within the incubation period of the disease; so that those who developed the disease within four days must have been infected before the inoculation. We may say, therefore, that no animal which was not already infected subsequently developed the disease after inoculation, in that outbreak. On the other hand, of about an equal number of animals similarly exposed to infection, and not inoculated, one-half developed the disease, and half of those died; making a mortality of 25 per cent. of the uninoculated animals. These results were so conclusive that the Government of India arranged to turn out 100,000 doses of serum a year. Many thousands of inoculations have been done, year by year, since; and, I think it was last year, a second laboratory was sanctioned in the south of India, because the one laboratory is not sufficient to make enough serum—the results have been so satisfactory.”

The following questions were put and answered:—

Q. Could you tell the Commission anything about the working of the Kasauli Institute: do you know anything about it?—*A.* I know something about it. I have not worked there myself. Originally, private subscriptions were raised in order to start a Pasteur Institute, so that the treatment of hydrophobia could be carried out in India, and the very serious delay of people going off to Paris could be avoided. When I went out to India first, in 1893, subscriptions were being raised. And a year or so later, one of the anti-vivisection societies, I do not know which, sent out appeals to all the native Rajahs and people

who had subscribed to this Pasteur Institute in India, or had promised to subscribe, to induce them to withdraw their subscriptions. However, Surgeon-General Harvey, at the Indian Medical Congress in 1894, gave an address on this question of anti-vivisection, which neutralised that effort. Later on, it was found that sufficient subscriptions could not be raised in that way; or, at least, before sufficient subscriptions were raised to do it in a private manner, the Government took it up; and I believe they gave a contribution equivalent to the amount which they had been spending on sending Europeans to Paris to be treated: and in that way the Kasauli Institute was started. The results are considered to have been so valuable, that within the last year a second Pasteur Institute has been started in the South of India, on account of the enormous area which has to be covered.

Q. Do you know the results of the treatment at the Institute?

A. The figures are practically identical with those of Paris itself; the results have been very favourable.

Q. Hydrophobia is not an uncommon disease in India, is it?

A. It is quite common.

DR. C. J. MARTIN, D.Sc., F.R.S., *July 10, 1907*

For Dr. Martin's evidence on Tropical Diseases, see Chapter VII. He gave evidence, also, on diphtheria antitoxin, and on the preventive treatment against typhoid fever.

Diphtheria Antitoxin

Dr. Martin gave an account of the experiments which led to the discovery of the antitoxin, and described its pre-

paration : he then gave a short summary of the good results obtained with it in Paris, New York, Vienna, Buda-Pesth, St. Petersburg, and elsewhere. He emphasised its great value in severe laryngeal cases of diphtheria. In 8,927 laryngeal cases treated without antitoxin, the mortality was 71·6 per cent. : in 2,374 similar cases treated with antitoxin, the mortality was 36·6 per cent. (Goodall, *British Medical Journal*, 1899, vol. i. p. 197). Dr. Martin went on to show, from more than 20,000 cases, the supreme importance of giving the antitoxin at the very beginning of the disease. Among 8,003 Chicago cases, treated with antitoxin, the results were as follows :—

Day of disease . . .	First.	Second.	Third.	Fourth.	Later.
Number of cases . .	608	2,063	2,802	1,496	1,034
Mortality per cent. .	0·32	1·66	3·64	11·03	21·08

(*Biennial Report of Department of Health, Chicago, 1904-5, p. 137.*)

He next alluded to post-diphtheritic paralysis : and pointed out that this result of the disease occurred mostly among those cases which did not get the antitoxin till late in the disease, or did not get a sufficient dose of it.

He summed up the facts of diphtheria antitoxin as follows :—

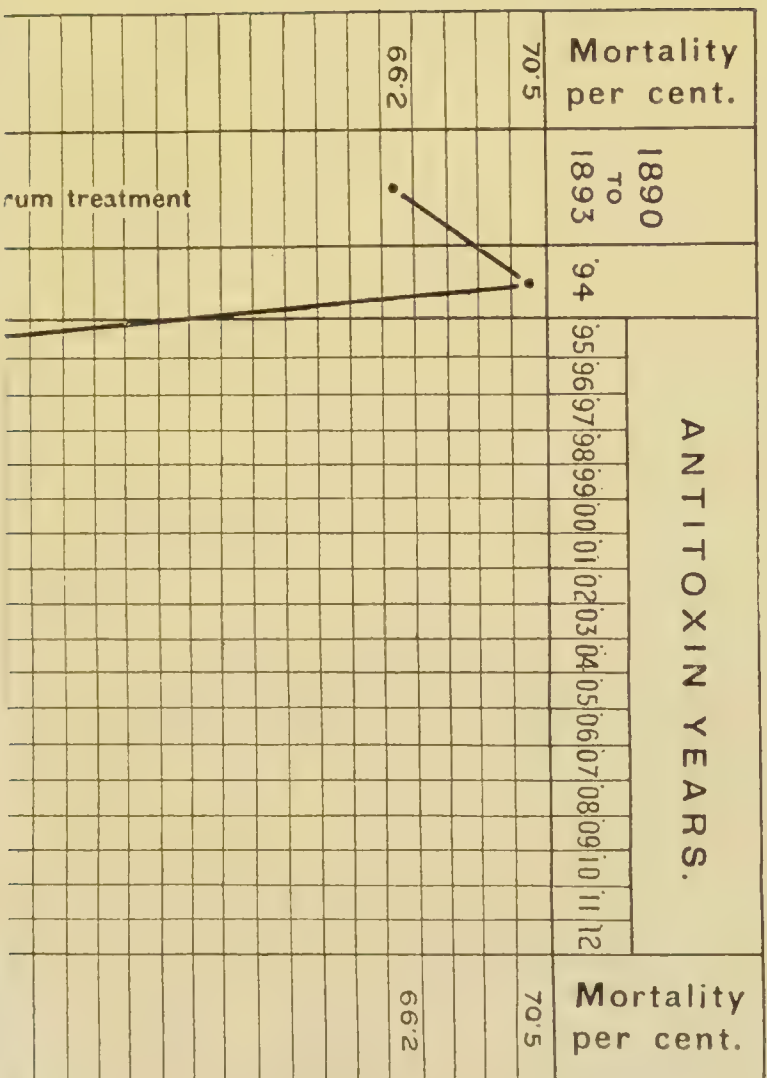
(1) Diphtheria can be cured with certainty, in animals, by treatment with antitoxin.

(2) The earlier the treatment is applied to an animal, the less the quantity required, and the more certain the cure : and, if the administration of antitoxin is delayed beyond a certain time, the treatment is ineffectual.

(3) A reduction of case-mortality of diphtheria to one-third or one-half occurred universally, coincidently with the introduction of the antitoxin treatment.

(4) In many places, a corresponding diminution in mortality in proportion to population occurred ; but in

Tracheotomy cases.



some countries this has been masked by a simultaneous increase in the prevalence of the disease.

(5) A similar fall in case-mortality amongst the severest forms of diphtheria, viz. those necessitating tracheotomy, has been universally experienced: so that the apparent beneficial results of serum treatment cannot be explained by the supposition that after 1895 (the year of the introduction of antitoxin) a milder form of the disease was suddenly and universally encountered.

The beneficial results of serum treatment are greater, the earlier in the course of the disease the treatment is applied: and, after the fourth or fifth day, the administration of antitoxin produces little or no influence upon the case-mortality. This is absolutely in accord with what was known of antitoxin from the results of experiments upon animals.

Preventive Treatment against Typhoid Fever

Dr. Martin pointed out, that the loss to our Army in India, by deaths from typhoid fever, amounts to half a battalion a year, apart from the pecuniary loss to the State through sickness and invaliding. In war, the loss, often, is heavier from typhoid fever than from wounds. "A method of preparing a vaccine against typhoid fever was devised by Wright and Semple in 1896. They satisfied themselves of the innocuousness of their vaccine by experiments, first upon themselves, and subsequently upon their friends." The first inoculations on a large scale were those at Bangalore, Rawul Pindi, and Lucknow, November 1898 to March 1899. Among 2,835 inoculated, there were 27 cases of typhoid, with 5 deaths; among 8,460 not inoculated, there were 213 cases, with 23 deaths. Later, came the instance of the 15th Hussars in India (October 22, 1899 to October 22, 1900). Among

360 inoculated, there were 2 cases, with 1 death; among 179 not inoculated, there were 11 cases, with 6 deaths. "The statistics concerning the campaign in South Africa, which have been collected together by the Army Medical Department, indicate a substantial diminution, both of the incidence and deaths from typhoid fever, amongst the inoculated." In 1905, came the instance of the 17th Lancers in India. The total strength of the regiment was 514, of whom 150 were inoculated. In October, whilst the regiment was still in camp, typhoid fever broke out, and 46 cases occurred. "Of these, 45 were amongst the uninoculated, and 1 amongst the inoculated. The men, both inoculated and uninoculated, were all mixed up together in the camp, and exposed to the same chances of infection. The value of anti-typhoid inoculation can no longer be regarded as doubtful; and considerable effort is being made by the Commander-in-Chief in India to secure the inoculation of as large a proportion as possible of the British Army in India."

NOTE.—Two sets of facts, later than the date of Dr. Martin's evidence, may be given here, to show the value of the preventive treatment against typhoid fever.

I

The Journal of the R.A.M.C., February 1909, says: "Sir W. B. Leishman has published the results of anti-typhoid inoculation in the Army up to June 1, 1908. The total number of men inoculated was 5,473, amongst whom 21 cases (3·8 per 1,000) and 2 deaths occurred. The number of non-inoculated was 6,610, with 187 cases (28·3 per 1,000), with 26 deaths. The case-mortality was 9·5 per cent. in the inoculated; and, in the non-inoculated, 13·8 per cent."

II

The President of the United States, on May 4, 1911, at a banquet given in his honour by the Medical Club of Philadelphia, spoke of the medical work done in Cuba, Porto Rico, the Canal Zone, and the Philippines. He went on to speak of anti-typhoid inoculation in the United States Army :

"We have a division of 18,000 men in Texas and California. They have been there for two months, living under canvas and in a country soaked with rain and deep with profanity-provoking mud. But so effective have been the regulative and preventive methods adopted to reduce sickness, that the percentage of sick men is less than it was in the posts from which these men were mobilised.

"I need not recall the dreadful record of sickness from typhoid fever in the camps at Chickamauga and other camps established during the Spanish-American War. The percentage of typhoid cases was so high that it is hard to believe. Of 120,000 men, there were 20,000 cases, with a case-mortality of 7 per cent. Of the volunteer regiments mobilised during the Spanish-American War, 90 per cent. became infected with typhoid fever within eight weeks from the date of mobilisation.

"To-day, two months after mobilisation, with the modern health regulations and by the use of vaccination against typhoid, not one case of typhoid fever has appeared in the entire force, except that of one teamster, who was not vaccinated. It is hard to credit the accuracy of such a record. But, as I have it directly from the War Office, I can assert it as one more instance of the marvellous efficacy of recent medical discoveries and practice."

The President of the United States has been pleased to give permission to the Research Defence Society to publish this Address in pamphlet form.

PROFESSOR GLAISTER, M.D., *October 23, 1907*

Dr. John Glaister, Professor of Forensic Medicine and Public Health in the University of Glasgow, and Medico-legal Examiner to the Crown in Glasgow and the West of Scotland, said that his attention had been particularly directed to the subject of poisons and of food-poisoning. He had taught on such subjects for twenty-five years. He was asked, "Do you consider that it is necessary to make experiments upon animals for the purpose of making these investigations about poisons?" He answered: "I have no doubt about it. I think it is essential." He gave, as an example, the Lamson case, in which the poison used was aconitin. Again, it was absolutely essential that animals should be used for experiment in the investigation of cases of food-poisoning. Again, experiments on animals were necessary in the study of the great outbreak in Scotland of cerebro-spinal meningitis: the need was, to discover the way in which the disease was spread. Asked what was his experience with regard to cases of criminal poisoning, he answered, "I think I can safely say that there are a certain number of alkaloids, which are so poisonous that I do not think any chemical means could possibly detect them, of which we know comparatively little now, and which must be investigated by the agency of animals." He added that our knowledge of antidotes, with respect to a large variety of mineral and vegetable poisons, had been derived from experiments upon animals of various kinds; and he named mercuric chloride, tartar emetic, lead, prussic acid, hemlock, laburnum, strychnine, and digitalis. Asked about ferric oxide as an antidote to arsenical poisoning, he agreed that its action was known previous to Watts's experiments in 1861: but he had no doubt at all that these experiments had much

to do with making the use of this antidote practically universal.

Professor Glaister went on to speak of the Act, and drew the attention of the Commission to the difficulty of knowing whether certain observations did or did not come within the scope of the Act. He thought that the heads of Departments should be able to obtain a less restricted form of Certificate A: "So far as I can see, the Act would then be quite excellent." Asked about serum-therapy in cerebro-spinal meningitis, he said that it had not been so successful as in diphtheria. "I am quite convinced that nothing has made such a vast alteration in the death-rate from diphtheria as the serum-treatment of diphtheria. I do not think that any medicinal treatment could have been devised which would ever have effected what this serum-treatment has done for diphtheria." He added that the preventive treatment against anthrax, also, had proved an enormous benefit to the world.

NOTE.—The Research Defence Society has published a paper by Dr. Gardner Robb, of Belfast, on the value of the serum-treatment of cerebro-spinal meningitis. It gives facts more recent than Professor Glaister's evidence. The use of Flexner's serum-treatment has brought down the death-rate of this terrible disease from 75 to 30 per cent. "My own experience has been," says Dr. Gardner Robb, "that of 275 cases under my care in Hospital before the use of the serum was commenced, 72·3 per cent. died: while, of the 98 cases treated with the serum, 29·6 per cent. died. No selection of cases was made: *every* case sent into Hospital since September 1907 has been treated in this way. . . . Great as this change in the death-rate has been, it is not more striking than the improvement in the course run by the

cases : for, whereas it was common to have cases running on into weeks and even months, such cases are no longer met with."

PROFESSOR WOODHEAD, M.D., LL.D., *November 12, 1907*

Dr. G. Sims Woodhead, Professor of Pathology in the University of Cambridge, and a member of the Royal Commission on Tuberculosis, gave evidence on behalf of his University. He said that the bulk of his work had been in connection with the diagnosis of diseases, especially in connection with diphtheria and tuberculosis; and also in connection with the preparation of antitoxins. In addition to this, he had done work on the healing of wounds, the organisation of blood-clot in vessels, the action of variolous vaccine, and the manufacture and testing of toxins and antitoxins, especially in connection with diphtheria.

Q. In connection with all those matters, have you made experiments on animals?—*A.* Yes, a very large number.

Q. And you have probably formed a very definite opinion one way or the other as to the value of experiments on animals in such matters?—*A.* A very definite opinion.

Q. What is your opinion?—*A.* My opinion is that a great part of the work that has been done recently could not possibly have been done without having recourse to experiments on animals.

Q. Does that apply to all these different inquiries that you have just been mentioning?—*A.* Just so: to each one.

Q. Has your work on the Royal Commission on Tuberculosis confirmed you in that view?—*A.* Entirely. It would have been impossible even to undertake the work if we had not been able to use experiments on animals.

Asked about diphtheria antitoxin, Professor Woodhead

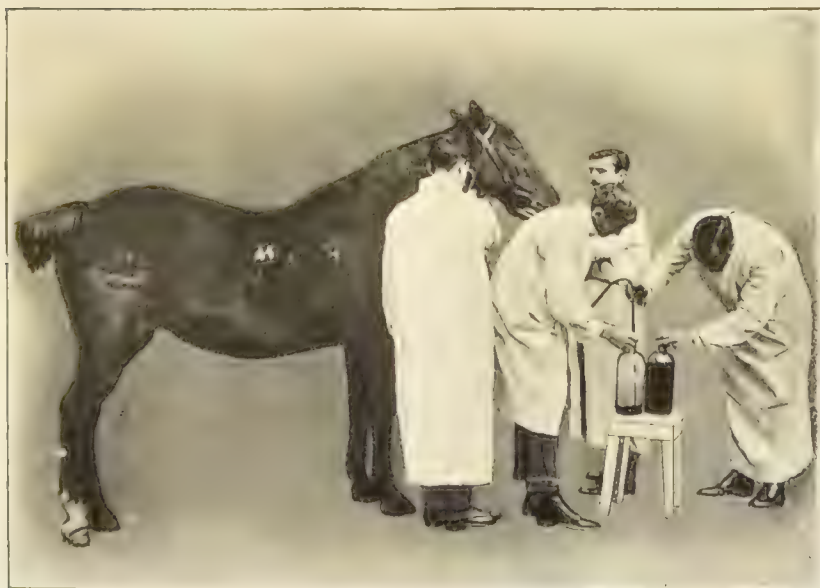
DIPH'THERIA ANTITOXIN

I



Immunising a horse with graduated doses of toxin, injected through a hollow needle put under the skin.

II



Drawing blood from a superficial vein. The serum of the blood contains the antitoxin.

said that he had studied it for the Metropolitan Asylums Board for four or five years, and had seen the commencement of the work. Asked whether the improvement in the case-mortality of the disease could possibly be attributed to a decrease in its virulence, and not to the use of the antitoxin, he described what had happened at two of the Board Hospitals, where the death-rate in cases of post-scarlatinal diphtheria had fallen from 45 per cent. without the antitoxin to 2 per cent. with the antitoxin: "The fall was so tremendous that it could not be accounted for by anything else. The cases were treated at once with the antitoxin. If the same set of cases had been allowed to go for some time untreated with the antitoxin, I have not the faintest hesitation in saying that there would have remained a very high mortality." He went on to speak of his own experience of the antitoxin: "You see a patient almost livid, breathing heavily, with a very high temperature, in great discomfort, and unable to sleep. You give that patient a dose of antitoxin: and in three to four hours he is breathing easily, the skin is acting, and the patient is comfortable, and he goes to sleep. That, to my mind, is even more striking than any statistics that one can bring forward.

"I may mention that, in our experiments on animals, carried out in connection with the testing of the potency of the antitoxin, an enormous dose of toxin may be neutralised by a dose of antitoxin; and you may see that animal recover without turning a hair, if one may put it in that way: never losing its appetite, never losing in weight, but simply going on as would a normal animal into which no toxin has been introduced.

"Moreover, we found that the more antitoxin was given in any hospital, and the earlier it was given, the greater was the diminution in the mortality. Everything else

being constant, the greatest fall in the death-rate took place where most antitoxin was given in the earliest stages of the disease.

“Our medical men are using more and more antitoxin every year. The worse the cases are, the more anxious they are to give the antitoxin. I said before, the very best results are always obtained when the antitoxin is given at an early stage, before the poison gets a thorough hold of the patient and damages the tissues. If you can give it early, the death-rate from diphtheria should be practically nil. The only cases that should be allowed to die are those that do not come under treatment until too late, or the few very virulent cases in which the poison seems to be so plentiful and so active that the patient is, as it were, stunned.

“In 1895 we only supplied 1,200,000 units of antitoxin to the whole of the hospitals under the Board; in 1896 we sent out 25,500,000 units; and in 1897 we sent out 60,250,000 units.”

The following questions were put and answered:

Q. Is the use of the antitoxin based upon the presumption or proof that the Klebs-Loeffler bacillus is the cause of diphtheria?—*A.* Yes.

Q. We have been told that, in 20 per cent. of the cases of diphtheria, that organism is not found?—*A.* I think that is quite possible, because in many of those cases you have, really, the Klebs-Loeffler bacillus crowded out by other organisms: and you get a condition which commences as slight diphtheria and ends as a septic sore throat. The one organism is practically crowded out by others.

Q. We have also been told that it is often found in healthy throats?—*A.* Certainly; I have found it in the throats of healthy nurses who have been attending cases of diphtheria.

Q. And even in cases which have not been in contact with diphtheria?—A. I have not had the opportunity of examining many such cases.

Q. You have not looked for it there?—A. No; but I have no doubt that organisms similar to it have been found: these, however, do not give the same toxic reactions. There are certain organisms resembling the diphtheria bacillus; but you can always distinguish between the two by determining whether they produce toxin or not.

Q. You mean the Hoffmann bacillus?—A. Yes.

Q. Has it not been alleged that in something like 15 per cent. of healthy throats the true Klebs-Loeffler bacillus has been found?—A. I certainly think it is found far more frequently than is realised; but I also think that it does not attack the throat until there is some slight inflammation, some slight breaking down of the tissues.

Asked about the use of diphtheria antitoxin as a prophylactic, in cases where there had been contact with diphtheria patients, Professor Woodhead said that it was distinctly valuable. Epidemics of the disease had been stamped out, by this prophylactic method, in Cambridge and in Colchester.

Asked about yellow fever, he said that the results obtained in Havannah were marvellous. They were entirely the result of the employment of agencies for killing the mosquito.

PROFESSOR OSLER, M.D., F.R.S., *November 20, 1907*

Sir William Osler, Regius Professor of Medicine in the University of Oxford, and sometime Professor in the Johns Hopkins University, Baltimore, gave evidence, first, on yellow fever and malaria (see Chapter VII). He went on to speak of the discovery of the action of the thyroid

gland, and the treatment of myxœdema and sporadic cretinism.

The Use of Thyroid Extract

“I thought it would perhaps interest the Commission, (as there are diseases in which I have been specially interested, and have had an unusually large experience), if I were to speak of them; because our knowledge of the methods of cure has come so directly from experiments on animals. We have learnt, through experiments upon animals, that the total removal of the thyroid gland was followed by a group of symptoms resembling cretinism and myxœdema. Gull showed us, and Ord showed us, that a spontaneous malady occurred in man, of the same character. The surgeons taught us that occasionally, following the total extirpation of the gland for goitre, the same symptoms occurred. Then, for very many years—centuries indeed—this condition of cretinism has been known in certain regions in which goitre is endemic, as in Switzerland. All of these conditions, particularly the sporadic cretinism and myxœdema, may be cured completely and permanently relieved by the administration of a powder of the thyroid gland.

“I do not know that there is a single step in our knowledge of this method of treatment—which in some ways is the most phenomenal that has ever been introduced—which has not been the result of experimentation on animals. If you will allow me just to show you here an illustration—striking as this is, it is even more striking in a woman—take a woman, for instance, who with advanced myxœdema may be reduced to a condition of imbecility, dementia, to a simply frog-like or toad-like caricature of her former self, and in a hopeless, helpless condition. Within six months that woman may be per-

THYROID EXTRACT



Case of Sporadic Cretinism, 1 year and 11 months.
BEFORE TREATMENT WITH THYROID EXTRACT.



Same case.

AFTER TREATMENT WITH THYROID EXTRACT.

From "Sporadic Cretinism in America," by Sir William Osler, Bart.,
M.D., F.R.S. Transactions of the Fourth Congress of American
Physicians and Surgeons, New Haven, 1897.

fectly well. And she stays well. Here, for instance, is an illustration (*producing a set of photographs*) of what may be done in eleven months. Here is a typical little cretin child, between two and nearly three years of age, unable to walk, unable to talk, with a pot-belly and prominent navel, with no sign whatever of intelligence about its face ; in fact, showing the typical apathetic, dull condition that characterises these cases. And here you see the successive changes in that child in the course of eleven months. You see I have marked it here, after two months' treatment, after three months, after five months, after seven months, and at eleven months (*handing round the photographs*). That is practically a miracle. That child was doomed to hopeless imbecility. And I have had a whole series of such cases : eighteen or twenty in all.

"What I feel, of course, is that such a thing as that, such a transformation as that which I have shown to you, is such a tremendous gain to humanity—just think what it must be to an individual who had a child in that condition—that it is impossible to put against it the lives of a certain number of dogs sacrificed. I do not think that the two can be weighed together. And when you think that that has been done in hundreds and hundreds of cases, and will be done, and that these people remain well, I think it is one of the strongest evidences in favour of the benefits which have been derived from experiments on animals that we can possibly have."

In reply to further questions, Professor Osler agreed that the association between cretinism and absence of the thyroid gland had been observed long before Sir Victor Horsley's experiments were made. "The disease of myxœdema was well known, and cretinism was well known; but there was no suggestion that it could be treated, until the results of Sir Victor Horsley's work were known."

The Work of Pasteur and Lister

Later, the following questions were put and answered :

Q. We were told by a witness the other day, who has had considerable surgical experience, that Listerism is now broken down and discredited.—*A.* Where did you produce that gentleman from—Hanwell?

Q. I will not mention his hospital, but he said that the antiseptic treatment had been absolutely discarded, and that the aseptic treatment was a reversion to the old pre-Listerian days.—*A.* It is the difference between tweedledum and tweedledee. They are both applications of the same principle.

Q. There being a slight difference in the one, due to increased knowledge?—*A.* Yes; it does not make any real difference.

Q. The witness also denied that the bacilli or protozoa which are found in inseparable connection with certain diseases have any causative effect; and he put his view rather strikingly. He said you might as well say that trout or salmon in a river were the cause of the river, as that the bacilli or protozoa were the cause of, say, Malta fever, or malaria, or sleeping sickness. Perhaps I might read you some words of his?—*A.* I think you might spare me. I decline to listen to twaddle of that sort. I would not answer a question of that kind. It is not the opinion of any man whose opinion is worth listening to, who has paid any attention to the subject, who has studied the subject, and who knows the conditions as they exist.

Q. Does his opinion represent anything of what might be called a school of medical thought?—*A.* Not at all.

Q. Or is it a merely individual eccentricity?—*A.* It represents the school of the back numbers.

PROFESSOR ROSE BRADFORD, M.D., D.Sc., F.R.S.,
November 26, 1907

Sir J. Rose Bradford, Physician to University College Hospital and the Seaman's Hospital, Member of the Tropical Diseases Committee of the Royal Society, and Chairman of the Sleeping Sickness Committee, gave his chief evidence on the value of experiments on animals for the early diagnosis of disease in man; for example, in the early recognition of phthisis, the recognition of tuberculosis of the bladder or kidney, and the early recognition of diphtheria and of typhoid fever. He was then asked his opinion as to experiments on dogs:—

Q. The last witness that we had was one who belonged to those who protest very strongly against dogs being used for these purposes. Are there any purposes for which you consider the dog is essential?—*A.* I think that it is very difficult to carry out experiments on the circulation in other animals than the dog. When I say that it is very difficult, that is perhaps not quite the correct word to use. But the dog is the animal in which the circulatory apparatus has much more resemblance to that of man than in the case of many other animals. That is one instance, I think. Then, of course, the digestive processes, and the nutritive processes generally, to which you have already alluded, is another illustration.

Q. Would you include, in the circulation, the lymph system—the lymphatics?—*A.* Yes; I think so. I have not yet myself worked at the lymph system; but I certainly think I would include it.

Q. If dogs were excluded from experimentation, what would be the result to physiology? Many people think that they should be excluded; and I wish to know what, in your opinion, the effect would be.—*A.* I do not wish

to use exaggerated language, but I am bound to say that I think it would cripple physiology in this country.

Q. To a great extent, or to a small extent?—*A.* To a great extent.

Q. As a matter of research?—*A.* As a matter of research; and, amongst other things, as a matter of direct practical value to man. The two things go together, I mean; they cannot be separated.

Professor Rose Bradford was then asked his opinion as to demonstration by experiments before students. "I think," he said, "that it is essential for the proper education of students that there should be demonstrations. I understand by demonstrations, of course, demonstrations on anæsthetised animals that cannot recover from the anæsthesia. My view about that is, that it is quite impossible for a student to get really a correct knowledge of any fundamental phenomenon in physiology, such, for example, as the beat of the heart, without really having seen the heart beating. I do not think that that kind of real knowledge can be acquired from reading. If you take two students, one of whom has been told the stimulation of a nerve in the neck stops the heart, and the other has seen the heart stop with the nerve being stimulated, I do not believe that those two students have got an equal knowledge. I know that I never had what one might call a true knowledge of the thing until I had seen it."

He was then asked about Mr. Graham's statement (see Chapter X): "Dr. Rose Bradford cut into the ears of dogs, destroyed the tympanic plexus, scraped out the middle ear, and poured in pure carbolic." He explained that the experiments were made for the study of the nervous regulation of the secretion of saliva. The operation was done under anæsthetics; one drop of carbolic acid was allowed to run in, and was then wiped up with cotton

wool, and the wound was closed. "It is quite untrue, of course, to say that they lived in a state of acute suffering. There was no evidence of suffering at all. Carbolic acid is a very powerful local anæsthetic. The experiments entailed no more suffering than obtains in the healing of any healthy wound."

He was then asked about Mrs. Cook's statement as to his experiments on the kidney. "The object of these experiments," he said, "was to see whether it was possible to produce in animals, by diminishing the amount of kidney substance, a condition at all analogous to what is found in human Bright's disease; that is to say, whether it was possible to produce a slow poisoning at all akin to that seen in Bright's disease: for the purpose, I need not say, of investigating Bright's disease. A number of facts were observed in the course of these experiments; perhaps the most important being that nothing at all akin to Bright's disease was produced by the diminution of the kidney substance; no such condition as that known as human uræmia was produced.

Q. Would you tell the Commission—if you remember at this length of time—exactly what did become of these dogs? You see, in that paragraph, Mrs. Cook accounts for nine out of the forty-nine; she does not tell us what became of the other forty.—*A.* I should be very pleased to put the facts before you. You have had a copy of the published paper, giving the facts as regards each one, of course. To summarise the thing shortly, I would say that a great number of these dogs lived, of course, for very prolonged periods, in perfect health.

Q. Without apparently suffering?—*A.* Yes.

Q. They were in the condition of a man who has had an operation on his kidney?—*A.* Yes: they were perfectly well. Some lived, for example, for two years; and they

were killed, not because they were ill, but because they served no useful purpose to keep any longer. Others wasted; they passed large quantities of urine, and got thin, but they suffered no more than that. They did not suffer at all, of course, except that they were thin. Others, where the quantity removed was greater, got so thin that they became extremely weak, and no doubt ill; they vomited occasionally; and those animals were killed immediately—there was no object in keeping them, of course. Then some of them, as stated by the witness, died within a few days of the operation. I remember this one alluded to here as having ‘lingered thirty-six days,’ because it was a very remarkable one. That animal lived for thirty-six days, and died suddenly, I do not know from what cause; but it is quite easy to give you the full facts as regards each individual one, if they are wanted. They are fully published.

Q. So that only two died from blood-poisoning as the result of wounds, and one in four days? That is what she says.—*A.* Yes, I have no doubt that is true; but I should have to look it up to verify it.

Q. And the others were much in the condition of a man who has had a successful operation performed upon his kidney, after he recovers?—*A.* Yes.

Q. That is to say, it is not likely that they were suffering tortures or pain?—*A.* There is no truth in the statement that they were suffering tortures or pain. And the only ones where there was evidence of the wound going wrong in the first few days after the operation were killed, of course.

Asked again whether there was any reason to suppose that the experiments on the secretion of saliva had given rise to great pain, he said: “I have no hesitation in answering that question, because I know that these dogs ran

about just like normal, healthy dogs ; and they showed no signs of pain." In reply to further questions about the experiments on the kidney, he answered that he would say, in respect of some of the dogs, that there was discomfort after the experiment. "I should not say that there was pain. But there was, in those animals in which there was the smallest amount of kidney left, a very considerable physical weakness and inability to take food, and therefore I have no doubt that there was discomfort. There was never any evidence of local pain, and there was no reason for supposing that there should be."

DR. HEAD, M.D., F.R.S., *November 27, 1907*

Dr. Henry Head, Physician to the London Hospital, gave evidence with special reference to experiments on animals for the study of diseases of the nervous system. "All rational diagnosis and treatment," he said, "must be based upon knowledge of the structure and functions of the nervous system. Sixty years ago, little was known of its structure, and still less of its functions." He read a passage from Sir Thomas Watson's lectures, 1845 : "the structure of the nervous system has no perceptible or understood subservience to its functions. We do not discover, in the mechanism of this system, that adaptation of means to an end which is so conspicuous in many other parts of the body." This passage showed the high-water mark of knowledge sixty years ago. "The whole of the knowledge which we now possess," said Dr. Head, "of the structure and functions of the nervous system, is due to the close co-ordination of the results of animal experiment and the consequences of localised disease. The anatomist can help us very little. He cannot even say whether the biggest tracts visible to the naked eye conduct upwards or downwards. The beginning of our present knowledge

came with Waller's discovery, that a nerve degenerated when separated from its nutritive centre. This law was the direct outcome of experiments on animals; and its application to the brain and spinal cord has been responsible for the greater part of that knowledge we now possess of the structure of the nervous system. The nerve not only loses all its functions, but undergoes retrograde destruction. When a nerve is separated from its nutritive centre, it becomes gradually functionless; but, also, it becomes gradually destroyed entirely, so that no nervous matter is left behind. Let me explain, by a simile, how the anatomy of the nervous system depends upon experiment. I am not talking of the functions at all; I am talking simply of the anatomical structure. Let me just take a simile. Imagine a wall covered with creepers arising from several stems. If we wished to know from which of these any one branch takes its origin, we could cut one stem; and every leaf arising from it would die, marking out among the healthy foliage the offshoots of the divided stem. This is the principle that has been used in tracing the paths in the nervous system. Gowers, by applying this method, discovered the ascending tracts in the lateral column of the spinal cord. By experiments on animals, a tract, or set of tracts, can be divided precisely: the animal is kept alive until degeneration has taken place, and is then killed. By suitable means, the dead parts can be coloured, so as to stand out clearly in the microscopical picture. The method, by which these dead structures are made to show-up clearly against the healthy parts, was discovered by Marchi from experiments on animals. Waller's law, and Marchi's method, applied to materials obtained from experiments on animals, and from disease or injury in man, are responsible for almost all our knowledge of the anatomical paths in the nervous system."

From the spinal cord, Dr. Head went on to speak of the brain. "Hughlings Jackson discovered, by long and laborious observation, that when certain parts of the surface of the brain were the subject of disease, certain movements were produced in the limbs. But these results remained a brilliant hypothesis till Hitzig in Germany, and Ferrier in England, showed, by simple, direct experiment, that stimulation of the surface of the brain in animals produced movements of the limbs such as Jackson had described. In spite of a generation of clinical observation since that time, many important details of localisation still remained undecided until Sherrington's and Grünbaum's experiments on the anthropoid apes. That is to say, that although Dr. Jackson, by the most brilliant intuition and most laborious observation, said that stimulation of certain parts of the brain marked by disease were associated with these convulsive movements, few, if anybody, believed him; because everybody said that it is impossible to stimulate the brain. They held exactly the idea that is given in Watson—that the brain was a sort of independent structure, in which no function was placed in one part or in another—that the brain was the seat of a sort of generalised function."

Dr. Head went on to speak about the relation of experiments on animals to experiments on self. "I hold very strongly," he said, "that no man has the right to be the subject of an experiment, unless the whole conditions surrounding that experiment are previously known as well as they can possibly be at the time. An experiment on man, if it occurs—self-experimentation—must be the culminating point in a complete series of thoroughly thought-out data, which can only be obtained by a long course of observations of disease, together with animal experiment." He spoke of the valuable knowledge which had

been gained by the present operation for the removal of the Gasserian ganglion. But of course there were other facts to be learned. "You cannot go on for ever learning one particular thing. I have not the slightest doubt that years hence, when our knowledge advances, we may go back to this, and learn more. What I wanted to prove was, that animal experiment has been necessary, not only for the study of the function (which is more easily, perhaps, understood), but also has been absolutely essential in the progress that we have made towards our knowledge of the anatomy of the nervous system. And then, it is not animal experiment only, but it is animal experiment closely interwoven with other forms of experiment." He gave as an example the interweaving of his own clinical study of referred pain with Sherrington's experiments on the distribution of the posterior nerve-roots. "Sherrington was working entirely independently. He went purely for knowledge. What is the distribution of the posterior roots? is what he asked himself; that is, How are they distributed? And he worked that out by animal experiment. That was undertaken entirely apart from any thought of disease, or anything to do with it; only it so happened that we were both influenced by Dr. Gaskell's ideas. And so he took the experimental side of the work, quite independently; and I wanted to see how far this idea could be borne out by the examination of cases of disease of patients.

"The next stage was to find out what happened if these parts that Sherrington had been working upon were irritated in man. That, of course, could not be done by an ordinary experiment, obviously. But it so happens that there is a disease which is known to everybody, shingles, which produces the most violent irritation of this posterior root." Dr. Head proceeded to describe

this stage of the inquiry, and added: "At the end of this stage, we at once saw how little we knew about the distribution of the nerves on to the surface of the body. Generations of anatomists had dissected the nerves of the human body, and it was thought that we knew all that was to be known about their distribution. Yet, if a workman cut his wrist and came to the hospital with one nerve divided, the actual anatomical facts could not explain what we saw in any single case. Any one who was honest, when a workman came to the hospital—any physician or surgeon who saw him, and was honest with himself—had to say that our knowledge, gained from dissection, tells us nothing with regard to the sensory condition of that man's hand. But there did not seem to be any way of working it out at first: and we were so impressed with the absence of knowledge on these points, that we set to work first of all to observe what actually happened when by accident various nerves were divided. We began to suspect that there were certain laws underlying the whole thing which had never been formulated. But, good as hospital patients are—and they are extraordinarily good from the point of view of many observations—it was obvious that no working-man could give the time required or the attention for a long and elaborate series of observations. Therefore, after everything had been thought out in the most careful manner, and after we had made several hundreds of observations on ordinary hospital patients, I determined to have three nerves divided in my own arm. This was done with the most successful results; the questions that we put by our operation were answered immediately, with a clearness that we could not obtain by any other means: and for five years we have been working out the results of that experiment. . . . We wanted to know what happens if you divide only the nerves that go

to the skin, and leave all the nerves which go to the muscles untouched. The day after the operation, that question was solved : and we thereby obtained a knowledge of an entirely new form of sensibility—a form of sensibility which had never been described before. A direct answer was given to a direct question. . . . Therefore the points that I want to make so strongly are, that in every inquiry into the structure and functions of the nervous system, observation of disease and experiments on animals are intimately interwoven : the one amplifies the other, and neither can be fruitful alone, because, as I have shown before, the very methods by which we examine disease are discovered from experiments on animals. The whole thing is all woven together like a fabric, and you cannot pull out one coloured thread without altering the pattern.”

In reply to further questions, Dr. Head described more fully the operation on himself, and explained how it had altered the former ideas about motor and sensory nerves. “One form of touch travels along the cutaneous nerves, and one travels along the deep fibres ; one form of pain travels along the cutaneous nerves, and one travels along the deep fibres. Heat and cold travel solely along the superficial, along the skin fibres. Localisation of a spot touched travels along the deep fibres. Localisation of a spot touched travels along the superficial fibres, but the localisation of a spot touched, travelling along the deep fibres, does not carry the power of distinguishing two points ; sensory impulses, travelling from the skin, carry the power of discriminating the two points.”

Asked about Sir Charles Bell's condemnation, in 1836, of experiments on animals, Dr. Head answered, “It is not a question really of whether I agree or not ; it is a question of pure fact. Sir Charles Bell's statement, there, is not worth any more than the statement of any other person

on that matter. The point is, How much more do we know now, in consequence of direct experiment? Bell did not know what we know. You cannot foretell. He only made that statement because it was his impression of his own time; that is a long while ago. We know a great deal more since then; and not only do we know a great deal more, but we also know how much more work we have got, so to speak, on our hands."

PROFESSOR LORRAIN SMITH, M.D., F.R.S.,

December 3, 1907

Dr. J. Lorrain Smith, Professor of Pathology in the University of Manchester, read a statement from that University, as follows: "The University is pleased to comply with the invitation of the Commission to give evidence regarding the experiments carried on in several of its departments. The laboratories of Physiology, Pathology, Pharmacology, and Public Health are registered for experiments on animals. Apart from original research, the experiments necessary for the public health investigations, required by the sanitary authorities for Manchester and the surrounding districts, are carried out in the Public Health laboratories of the University. The work carried on in the Public Health laboratories is highly valued by a large number of local authorities, representing about five millions of people; and Officers of Public Health find the reports issued from them of great value as regards the detection and prevention of the spread of disease. Some of the Hospitals also avail themselves of these reports, which are often very helpful in diagnosis. The University authorities have endeavoured to comply with the regulations imposed by the Act to the best of their ability. The laboratories are visited by the Inspector several times each year, without previous warning, and

the fullest information is given him regarding the researches which are being carried on. The Inspector, on his part, thoroughly investigates and examines all the animals under experiment. Any representation which he may make regarding accommodation for animals, etc., is carefully considered, and, so far as can be gathered, has been hitherto dealt with to his satisfaction. The University authorities consider that adequate care is taken to further those objects which the Act was framed to accomplish, and that the present legislative provision for controlling this type of investigation is sufficient. The University has never had reason to find fault with any of the licensees for the manner in which the experiments have been carried out. In view of the care which has been exercised in conducting this type of work in the departments of the University, and of the thoroughness of the administrative control by the Home Office, the University considers that further restrictive measures are unnecessary and undesirable."

Asked about the work of the University of Manchester, he explained that the local authorities had the use of his laboratory. This work for public health purposes was mostly concerned with diphtheria and tubercle; it might also include anthrax, tetanus, and food-poisoning. Mostly the animals used for these inoculations were guinea-pigs or rabbits. He had never known inoculations made in the eye for diagnostic purposes.

Dr. Lorrain Smith proceeded to speak of the value of experiments in pathology, other than bacteriological experiments. He took as examples the influence of adrenalin in the production of arterio sclerosis in rabbits, the study of the influence of oxygen in the production of pneumonia, the study of fatty degeneration, and the study of rheumatoid arthritis. He also referred to the

experimental study of the causation of certain diseases of the nervous system, such as general paralysis.

He was asked about certain experiments made in Belfast with jequirity. He explained that he, not Mr. Cecil Shaw, had done them. Asked whether these experiments had caused the rabbits a good deal of pain, he answered, "No, they did not. We did not cause the iritis; we did not succeed in causing iritis. It was a very minute quantity of jequirity, just the least speck of dust on the point of a camel-hair brush. No inflammation was caused, except conjunctivitis. In no case did we cause iritis."

Asked for his opinion as to the value of demonstrations by experiment before a class, he answered: "I could quite imagine their being of enormous teaching value. I know how I was impressed the first time I saw a physiological experiment myself. One of the things that stands out in my recollection of Professor Rutherford's teaching, almost more clearly than anything else, is the experiment of blood-pressure." Asked whether the animal, in that experiment, had been anæsthetised before the tube was inserted into the trachea, he answered that he was sure it was. He had never, as a student, seen tracheotomy on an animal except under an anæsthetic.

DR. SHORE, M.D., *December 11, 1907*

Dr. L. E. Shore, Fellow of St. John's College, Cambridge, and University Lecturer on Physiology, was asked about certain statements made by Colonel Lawrie (see Chapter X). He explained that the dose of morphia given in the experiments in question was thirty-six times stronger than appeared from Colonel Lawrie's *précis*. The animals were absolutely under anæsthesia before the operation began, and up to the time of death. They were absolutely unconscious of pain the whole time. Dr. Shore also referred

to his own experiences under anæsthesia. "On the two occasions that I had to undergo an operation, and was given an anæsthetic, I decided to see whether I could tell the length of time that intervened from the commencement of giving the drug to the period of unconsciousness, and I decided to do it by carefully counting my regular respirations. On the first occasion I was able to count ten respirations, and then I became absolutely unconscious. On the second occasion I could count twelve: that makes considerably less than a minute before unconsciousness supervened. The second point which those personal experiences led to is also interesting. In the middle of the operation I heard the surgeon make a certain definite remark about the operation, but I could feel no pain whatever. When the operation was completely over, and I recovered consciousness, some four hours later, I inquired of the surgeon, and he told me that the remark had been made when the operation had been in progress about half an hour. That experience, I think, points out that the sensation of pain is more easily abolished than such a sensation as hearing. I was supposed to be fully anæsthetised, but at the time when I heard the remark, no doubt the anæsthesia had been allowed to go light; but there was not the slightest sensation of pain, absolutely none."

V

SURGERY

SIR HENRY MORRIS, Bt., P.R.C.S., *May 8, 1907*

SIR HENRY MORRIS, President of the Royal College of Surgeons of England, read to the Commission the resolution of the Council of the College :

“The Council of the Royal College of Surgeons of England express the opinion that the Act of 1876 is sufficient protection against any abuse of vivisection, and earnestly hope that the progress of surgical science will not be hindered by further restrictive legislation.”

Speaking first of Lord Lister's work, “We could not,” he said, “have arrived at the knowledge we possess at the present day, had it not been for experiments on animals”; and he had no hesitation, as an experienced surgeon, in expressing his opinion that vivisection had been essential. He pointed out that Lord Lister, giving evidence before the Royal Commission of 1875, had said that experiments on animals had been essential to his work.¹ “It was by

¹ See Report of the 1875 Commission, p. 216. Lord Lister, then Mr. Joseph Lister, gave evidence November 1, 1875. He was asked by Mr. (Sir John) Erichsen : “Q. Am I correct in understanding that in all your various capacities, as practitioner of surgery and as a teacher of surgery, and also as a man who has been foremost

means of experiments on animals, whilst studying the phenomena of inflammation, that Lister ascertained many very important and fundamental facts. Thence he went on to the study of the blood, more especially the coagulation of the blood, thrombosis, etc. The facts ascertained in both these directions formed the basis of his subsequent work on the treatment of wounds. Lister, having come to the conclusion that the disasters which attended wounds were directly dependent on putrefaction of the discharges in the wounds, directed his attention to attempts to prevent this putrefaction; but, for a long time, without any marked success, because the causes of this putrefaction were unknown." Sir Henry Morris went on to speak of Pasteur's influence on Lister's work, Pasteur's experiments on fowl-cholera and anthrax, and Koch's work. "I would say that, as the result of the most intense study and work, Lister constantly improved his methods of wound treatment, with the most remarkable results in the prevention of septic diseases, as well as of certain diseases which at first he had not looked on as due to the entrance of bacteria, such as erysipelas and tetanus. But knowledge of these matters was only increasing slowly and imperfectly, till the appearance of Koch's work on the infective diseases of wounds. . . . Of course, the whole of the aseptic and antiseptic methods of the treatment of wounds is the outcome of this knowledge of the micro-organisms, in the putrefactive discharges, being the cause of the disease. The discovery of the causes of these diseases has led to the

in the advance of surgical practice and science, you have found experimentation on living animals necessary?—A. That has been so. Q. And that, through the medium of that experimentation—originally commenced, possibly, I think I understood you to say, with other views—you were gradually led to the development of that method of treatment which is now known as the antiseptic system?—A. Yes."

early and precise diagnosis of them, and to a more intelligible and effective manner of treatment than was previously possible. In the prevention of the spread of diseases, and in hygienic science, advances have been made owing to experiments on animals which could not have occurred had such experiments been prohibited. . . . The experimental study of pathogenic organisms has also led to the discovery of protective and curative sera of proved value and of great importance in treating a number of diseases; the most striking instance being diphtheria."

Sir Henry Morris added, that the present use of anti-septic or aseptic ligatures, also, had come from Lister's experiments on animals. Next, he spoke of the advance of operative surgery. "Since the danger of septic troubles has been removed, enormous strides have been made in surgery of recent years. Many operations which used to be looked upon as too dangerous to be undertaken, except under stress of necessity or as offering a last chance of saving life, are now performed with absolute safety; many new and important ones, which were formerly never dreamed of, have now become matters of successful and almost daily performance.

"Finally, whereas the mortality, even after quite simple operations, was at one time so appalling that the patients were often deterred from submitting to them, and the surgeon from performing them, operations of considerable magnitude are now undergone with the confidence which comes from knowing that the mortality is, in many instances, but a fraction of an unit per cent. To-day, the abdominal cavity of man is opened, every organ within is inspected and palpated for diagnostic as well as for operative purposes, the wound is closed, and the patient recovers from the effect of the anæsthesia, and takes a completely uneventful course towards convalescence. A

joint is the seat of a sudden and most painful inflammation, which is mistaken for an attack of acute rheumatic fever; it is, nowadays, punctured, the fluid is withdrawn and examined, it is found to be septic, and the joint is then at once laid open and drained: immediately, all pain vanishes, and the patient quickly recovers with a moveable and natural joint, instead of, as formerly, after a lingering illness—if at all—and with a stiff joint, a deformed limb, and suppurating sinuses discharging on the surface of the body. The disease technically known as osteo-myelitis, or diffuse abscess within bone, is no longer looked upon as due to exposure to cold, or as rheumatic; nor is empyema, or suppuration in the chest cavities, any longer attributed to access of air to the pleural surfaces; but both diseases are alike treated by the freest incisions and drainage, with the result that great suffering is relieved and thousands of lives which formerly would have succumbed to these diseases are saved. The foregoing are some illustrations of the beneficent results of the combined labours of Lister, Pasteur, and Koch.”

Cancer Research

“In the matter of cancer, it is important to point out that, before the establishment of research laboratories, and up to 1902, cancer had been studied almost exclusively from the clinical aspect, and by pathological, histological (microscopical), and chemical examinations of the dead cancerous tumours. The result has been that our ignorance as to the cause and origin of cancer, at the beginning of the twentieth century, was almost as profound as ever before. Hence the importance of studying living cancer-tissue by transplanting it from animal to animal; and mice were selected as the subjects of the great majority of the

experiments, because the disease occurs spontaneously and frequently in them.

“To keep the tumour-tissue alive, and to provide a sufficient and continuous supply of it, large numbers of inoculations are essential:

(a) Because many of these inoculations fail entirely.

(b) Because in many instances the tumours which arise and for a short time grow in mice subsequently atrophy and disappear.

(c) Because of the small amount of tumour-tissue obtainable from each mouse.

(d) Because the step-by-step investigation of the changes which take place at the site of inoculation entails the sacrifice of a large number of animals at every stage.

(e) Lastly, because the investigation of the secretions and body-fluids of cancerously affected, as compared with unaffected, animals requires the use of a large number of mice.

“If it be asked what, up to the present time, has been learnt or confirmed by all these experiments, the answer is:

(1) Positive knowledge has been gained that this mouse tumour, which is identical in nature with the human mammary tumour, grows by an inherent growth of its own, and is not due to a change in the tissues themselves of the animal into which it is engrafted.

(2) That cancer can, and does, not so very infrequently, disappear spontaneously.

(3) That the animals in which cancer has grown, and then disappeared, are immune to fresh inoculation.

(4) That a mouse can be rendered immune by the injection into its body of some of the blood of a healthy or an immunised mouse.

“It should be pointed out, in reference to these inocu-

lation experiments, that they are practically painless; or, if pain can be said to be caused at all, it is but slight and momentary, and to be compared in the human subject with the pain caused by a simple subcutaneous puncture: neither is pain suffered by the animal in which the transplanted tissue grows even into a large tumour. It is only in the exceptional cases, in which the tumour ulcerates, that any pain occurs; and, as this condition renders the tumour useless for further experiment, the animal is at once killed.

“Evidence as to the painlessness of these growing tumours is afforded by the activity of movement, sleekness of coat, and general healthy appearance of the mice; their indifference to pinching or pressure of the tumour, which is not innervated; and further, by the fact that when they have been kept in separate compartments of the same cage, they have eaten through the partition, mingled with each other, and a litter of young ones has been the result.

“Every cutting operation in these investigations is performed under complete anaesthesia, and precautions are always taken to secure asepticity and immediate healing of the wound.

“As regards other animals, inoculation experiments have been undertaken, to a very limited extent, on horses, dogs, rats, rabbits, frogs, and fish. Only in the case of the rat and the dog have inoculations been followed by the development of the disease; and to these the same remarks apply as in the case of the mouse.

“Any limitation placed upon the number of inoculations, whether in mice or in other animals, including anthropoid apes, to which resort may ultimately be necessary, will have a disastrous effect on the further development and the prosecution of cancer research in this country. It is at present impossible to determine how far the contrast,

between the success obtained in mice and the failure in other animals, is due to the discrepancy in the number of inoculations made.

"As a member of the Executive Committee of the Imperial Cancer Research from its outset, I have had frequent opportunity of witnessing the complete absence of suffering in animals the subjects of these cancerous tumours, and of observing the extreme care and attention given to all the animals, both in London and at the farm."

Sir Henry Morris went on to speak of the help given, by experiments on animals, to operative surgery: for example, the operative surgery of the brain, the gall-bladder, the stomach and intestines, the larynx, and the kidney. "Czerny, to investigate the question whether the larynx could be extirpated without death resulting, performed a series of experiments on eight dogs, all of which underwent total extirpation of the larynx. The first four died from the consequences of the operation. The last four survived and remained well. It having been thus shown that the larynx is not indispensable to life, and that the operation is feasible, extirpation of the larynx became a recognised surgical operation." In the surgery of the kidney, Simon of Heidelberg, in 1869, had been guided by his experiments on dogs to venture to remove one of the kidneys of a human being.

Next, the witness referred to the good results obtained with the preventive treatment against tetanus after certain lacerated wounds of the hand,¹ and to the value of Sclavo's serum in cases of anthrax. In the last three cases of anthrax at St. Bartholomew's Hospital, they had relied

¹ See footnote to Major Leonard Rogers's evidence, Chapter IV. See also Mr. Stockman's evidence, Chapter VIII.

upon the serum treatment alone, with speedy recovery of all three patients. "In several cases, of course, both in Italy, this country, and elsewhere—and especially in the earlier times of the employment of Sclavo's serum—not having confidence enough in the serum, surgeons excised the pustule as well as giving serum; but as they gained confidence with the serum, they have done away with the operative part of the treatment, and have been relying upon the injection of the serum alone."

He was questioned as follows on the surgery of the kidney:

Q. (*Colonel Lockwood*). You made your name, I think, which is a very great one, by experiments on the kidney, was it not?—*A.* I never made an experiment in my life.

Q. I do not mean on an animal, I mean operations—I apologise.—*A.* Oh, no need for that; I thought perhaps you meant experiments on animals.

Q. No, I meant operations?—*A.* Yes.

Q. Were those operations, which you have conducted so skilfully, and fortunately discovered—were you made to do them by experiments on living animals?—*A.* Personally, not at all. The operation that I did in the first place, and was the first to do, was done quite irrespective of any experiment upon animals; that is, the cutting of a stone out of a kidney. But that was not the case with regard to cutting a kidney out of the body: because Simon of Heidelberg, who was the first to do that, made a deliberate set of experiments, for the express purpose of knowing various things before he submitted a patient to the operation."

Sir Henry Morris also spoke of the value of demonstrations in physiology: "These exhibitions are of very great importance in teaching classes. I am not suggesting that there should be experiments on animals for the

purpose of teaching how to operate ; but I am suggesting that it is very important to retain the power that still exists on the part of physiologists to give a certain small number (and they always are but few) of demonstrations to classes who are learning the functions of the body : for instance, to know what the action of the heart is like, and what the action of the intestines is like, and the effect of influencing the organs through irritation or section of different nerves. There are a few things of that sort which can only really be fully and completely impressed upon the student by having practical demonstrations of them ; and they are performed in small classes (not in a great theatre), before a group of students, upon animals which are under an anæsthetic and are killed as soon as the experiment is over, and before they are allowed to come round from the anæsthetic. This is merely one of the things that concerns the teaching of the sciences of the profession, to which many of us attach a good deal of importance. I was a pupil of Dr. Pavy at Guy's Hospital. There were not many experiments ; perhaps only four or five in the course of a whole session : but I can say from personal experience how one was impressed by the sight, for a few minutes, of a thing of that sort, which no amount of reading or verbal descriptions or lectures would ever have given to one."

SIR HENRY SWANZY, P.R.C.S.I., *June 12, 1907*

Sir Henry Swanzy, M.D., President of the Royal College of Surgeons in Ireland, sometime President of the Ophthalmological Society, and Surgeon to the Royal Victoria Eye and Ear Hospital, Dublin, said that the unanimous view of his Council was, that experimentation on animals "is necessary for the advancement of medical and surgical

science, in that it has introduced great improvements and advance on our knowledge and our methods in the past, and that we may look forward to its doing similar work for the good of medicine and surgery, and of humanity in general, in the future." Questioned as to certain special certificates under the Act, he said that they did not object to the present limitations, but would not extend them. Questioned as to experiments performed for the acquirement of surgical skill, he answered, "I am not a general surgeon—as you understand—but all those with whom I have spoken, including some of the most distinguished men with us, are clearly of opinion that it would be greatly to the advantage of surgeons, and of those learning to be surgeons, if operations upon animals for the purpose of acquiring skill were permitted. I would extend it to students towards the end of their course, when they were almost surgeons, and when they were about to enter upon practice—where they would have to perform operations very soon upon the human animal. I agree with a witness who was here (Professor Starling) who made a very striking observation, which was, that he regarded the present law as immoral, because it did not permit these operations upon animals for the acquirement of skill." Questioned as to demonstrations before a class, he answered: "The view of my Council is, that it is desirable that students should be taught, when necessary, by means of experiments upon animals. There was one member of our Council—I wish to be perfectly candid—who seemed to be a little doubtful on this subject; but he seems to have altered his mind about it. I agree with the majority of the Council."

Sir Henry Swanzy went on to speak of the advantages gained through ophthalmic experiments on animals, for the science and practice of ophthalmic surgery. Cohnheim,

about 1870, had gained much knowledge, by such experiments, of the processes of inflammation. In 1880, Deutschmann had discovered that the ciliary body secretes the fluids of the eye; this discovery, which was made entirely by experiments on animals, had advanced knowledge as to keratitis punctata, and as to opacities of the vitreous humour, and had influenced the treatment of these affections. In 1873, Leber, by experiments on animals, had discovered the protective use of the endothelium of the cornea; and this discovery had been of great service to the practice of ophthalmic surgery, not only in the operation for cataract, but in the operation for transplantation of the cornea (von Hippel, 1877). In cases of *ulcus serpens*, advantage had been gained from the use of a serum-treatment (Römer, 1905). In cases of that very prevalent and very devastating disease, tubercle of the eye, the tuberculin treatment was exceedingly valuable; the tuberculin was useful both in treatment and in diagnosis (von Hippel, 1904). In cases of trachoma, the use of jequirity, which sometimes produced excessive reaction, endangering the cornea, could now be rendered safe, as a result of Ehrlich's and Römer's experiments on the production of immunity against jequirity.

In sympathetic ophthalmia, experiments on animals had given negative results; but they had been useful, because they had helped to clear away the former theories of this disease, and to found the present idea, that the disease is metastatic—"that is to say, when one eye is injured, micro-organisms set up their residence in it; and in the course of time, a few weeks, they extend into the general system; but they do no harm to any organ in the general system, except the second eye if they happen to go into it; and they appear to have some special attraction for the other eye, which is a very remarkable thing. If we knew

for certain that this view were correct, and if we knew, which we do not yet know, what the specific micro-organism is, we could then perhaps get a serum which, when an eye was injured, we could introduce into the patient's system, and immunise him, and prevent him from getting sympathetic ophthalmitis. And many a person who is going about blind now, if we had such knowledge would now be seeing. It is a very common disease; it occurs in shooting accidents, and all kinds of perforating wounds; we see it every day. The best thing we can do for people nowadays, when they come to us with an injured eye, is to advise them to have it removed. But very frequently they say that they would rather not; and they go off and come back again in a few weeks with serious inflammation in the other eye; and they go about with the help of a dog and a stick for the rest of their lives."

Von Graefe, about 1867, discovered that the disease of glaucoma could be cured by iridectomy, by the removal of a piece of the iris. "He discovered this cure, not by experiments upon animals, but by clinical observation; because, after all, every advance that we make is not made by experiments on animals. Although experiments on animals teach us a vast deal, yet other methods are also not forgotten." But it was by experiments on animals that Leber, in 1873, advanced our knowledge of the cause of glaucoma. Also, the use of eserine, as a palliative measure, was discovered by experiments on animals, by Fraser.

Later, the following questions were put and answered:

Q. (*Colonel Lockwood*). I understood you to say that you were in favour of demonstrating to students by means of experiments on living animals?—*A.* Yes, certainly.

Q. And you would be sorry to see them put a stop to?—*A.* I would, indeed.

Q. Do not you think that that knowledge could be arrived

at, as it was in your own case, as much by reading as by witnessing experiments?—*A.* The experiments which are shown to students relate to the great functions of the body, such as the action of the heart, peristalsis of the bowels, and such things as that. They certainly are things which ought to be taught to students in the most demonstrative manner possible.

Q. But I may take it—indeed, it is well known—that you yourself have arrived at considerable eminence in your profession, and yet you say you have witnessed no experiments.—*A.* It would have been a very good thing for me if I had.

Q. You think you would have been a still greater man?—*A.* I am sorry I did not see still more of them.

Q. Did you see any in Germany?—I saw the beginning of Cohnheim's experiments; and they certainly were something which impressed the nature of inflammation upon my mind more than anything I ever saw in my life. I only wish I had seen more. When I was a student these things were not to be seen, and therefore, of course, I was at a disadvantage.

In reply to further questions, Sir Henry Swanzy said that he thought the ascertaining of knowledge was to be put before the suffering of the animal; that was his own individual opinion: but he would always endeavour to the utmost to safeguard the animal from any pain, or even inconvenience, which could be avoided. Asked whether all parts of the eye were equally sensitive to pain, he said that they were not. There was no sense of pain in the vitreous humour (except under increased pressure), nor in the retina. Pain in the eye was mainly a question of the cornea and the conjunctiva: and the iris was very painful to be touched, and so was the ciliary body. In all ophthalmic experiments, involving operation, the animal ought to

be under a general anæsthetic, chloroform or ether: no doubt, in many instances, there would be suffering after the operation.

SIR VICTOR HORSLEY, F.R.S., *November 13 and 19, 1907*

Sir Victor Horsley, F.R.S., F.R.C.S., Consulting Surgeon to University College Hospital, and Surgeon to the National Hospital, gave evidence as representing the British Medical Association. He pointed out that the medical profession had subscribed, since 1874, more than £30,000 towards medical research. "The medical profession consider that every effort ought to be made to increase knowledge, for the prevention of suffering and disease affecting not only man, of course, but also the lower animals: and they consider that any one who opposes such pursuit of knowledge is acting immorally. To them, the moral duty is the pursuit of knowledge: and an immoral act is the obstruction of that pursuit of knowledge. I would compare, in this particular, the fact that of their own moneys they have subscribed this large sum of £30,000 for the pursuit of knowledge: while of the anti-vivisection party, Mr. Coleridge's Society, to take that alone, have collected from the public £86,000, and have not added one item of knowledge to us for the relief of suffering or the prevention of disease.

"May I also draw the attention of the Commissioners to the fact that several witnesses have spoken as though the anti-vivisection party were morally justified in accepting the knowledge gained by vivisection. May I point out to you that it has always been the feeling of the profession that the anti-vivisectionists were not morally justified in accepting knowledge thus gained. The profession would, in fact, regard it thus—that if the anti-vivisection party

consider that facts have, as it were, been immorally obtained from Nature, stolen from Nature, the persons who benefit thereby are practically receivers of the stolen facts. . . . We know, of course, that no one feels the pulse of a patient and derives any information therefrom without being indebted to vivisection. Morally speaking, it seems to me (I am only giving my own personal opinion, of course), that the anti-vivisectionists have no right to derive benefit from experiments on animals, holding the views that they do."

Sir Victor Horsley next spoke of the meeting at the Mansion House in 1889, in support of the preventive treatment against rabies. "I had the honour of acting as secretary of a Committee that was appointed by the Government to inquire into M. Pasteur's treatment: and, when the Committee was in Paris, M. Pasteur said to us, 'Why do you come here to study my method? You do not require it in England at all. I have proved that this is an infectious disease: all you have to do is to establish a brief quarantine covering the incubation period, muzzle all your dogs at the present moment, and in a few years you will be free.' . . . Immediately this point of universal muzzling was raised, the anti-vivisection party entered upon a virulent campaign against us, and held meetings, and endeavoured to show that M. Pasteur's discovery of the real infective nature of the disease was wrong: they revived the old ideas, that it was lunacy in dogs, that it was not a disease; and they revived also the particularly dangerous idea (from the public point of view) that it could spontaneously generate in the dog: whereas M. Pasteur had proved that the exact opposite was the case. I need not weary the Commission, but I would point out that we obtained partial muzzling, in spite of this opposition; and that that did so much good that then Mr. Walter Long

applied the muzzling universally, and established quarantine. First, they had the power to muzzle by counties: and, in the County of London, I showed, from the records of the Brown Institution, the laboratory of which I then directed, that the effect was wonderful in immediately cutting down our admissions of rabies, and that it also cut down another contagious disease, distemper. The effect of that was, that Mr. Walter Long, when he was at the Board of Agriculture, imposed a universal muzzling and brief quarantine: with the result that fortunately now the United Kingdom is free from rabies. . . . The action of the anti-vivisection party in that respect we consider to have been immoral. We consider that it was opposing the results gained by scientific research, that those results were of value to the human community, and also to the lower animals: and that that opposition was immoral."

The witness went on to speak of the value of demonstration experiments for the proper teaching of students. Certain facts should be demonstrated in physiology, pathology, pharmacology, and surgery. "All I can say is, that I have shown a very large number of experiments to students, that they have always evinced the utmost interest in them, and they have told me that the facts which the experiment illustrated have been imprinted upon their memory in a way that they could not have obtained by any other means. In fact, they have simply emphasised the statement of Professor Huxley before the 1875 Royal Commission, where he said, 'There is no teaching of physical science worth anything as thorough teaching, unless it is accompanied by practical instruction.'" Speaking of demonstrations in pathology, he spoke of the demonstration of epilepsy on animals under anaesthesia. "Epilepsy, for instance, can be produced experimentally with absolute fidelity, by the simple injection into the veins of a drop of

essence of absinthe. In 25 seconds you have a typical epileptic fit produced: and a student who has once seen it, and watched it develop through the body of the animal, never could forget, and never has forgotten it."

Q. (Sir Mackenzie Chalmers) Is that the ordinary absinthe of commerce?—*A.* It is the pure essence which is diluted for the drink of vermouth. It is extremely toxic.

Q. (Mr. Ram) Would the animal be under anæsthetics?—*A.* Entirely.

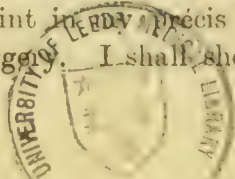
Q. The anæsthetics would not interfere with the development of the epileptic symptoms?—*A.* No.

Q. (Chairman) Nor would the animal be allowed to recover?—*A.* No.

He had never heard of any demonstration where the animal had not been put to death under the anæsthetic after the demonstration. He went on to speak of the teaching of surgery by the help of experiments on animals under anæsthesia. "The Commission are well aware that at the present moment the only practical teaching in surgery that a student receives, except by what amounts to his experiments on human beings, is from operations on a dead body: and those, from the anatomical point of view, are lamentably few in number. I am referring to the well-known fact, also, of course, that the supply of dead bodies is too scanty in London. But that operating upon a dead body is practically only the revision, for the student, of his anatomical knowledge. It does not teach him the science of surgery, that is, the operating on living tissues, which is a totally different thing. I say the texture, and the method of dealing with the live tissue, is quite different from that in dealing with the dead tissue; and, from the ethical point of view, it seems to me that it is not moral for students to gain their knowledge on man when they can perfectly well gain it on an anæsthetised lower

animal." Sir Victor Horsley described this mode of teaching at the Johns Hopkins Hospital in Baltimore. "It is conducted most strictly, like any other practical class, and the demonstrator sees to it. The different students are told off to act in different ways; one to act as anæsthetist, another to take the notes, another to do the operation, two or three more to act as dressers, and so forth. They are all told off, and they keep records of all their work. Then, in association with that point, is the very important question which has just been raised, of anæsthesia. I wish to draw the attention of the Commission to the fact that the risk of death from anæsthesia has always been justly looked upon as a great reproach; and I wish to express my personal opinion, that it is purely a matter of knowledge of the dose required, and that, as regards the education of students in anæsthetising patients, no one ought to be allowed to render a human being unconscious before he has had practice on animals. I wish to say that in reading this evidence which has been offered to the Commission, I am struck with the extraordinary ignorance which prevails, even now, on the question of anæsthetising animals. Over and over again, witnesses have spoken as though anæsthetising an animal was a totally different thing from anæsthetising a human being. It is nothing of the kind. We have even had revived before the Commission the absurd statement, made thirty years ago, that you could not keep a dog under chloroform for—I forget what the limit was—two hours, or something of that sort. You can keep it under chloroform for a week, if you only take the trouble. I have kept an ourang under ether for eight hours; and I would keep a dog, or any other animal, under an anæsthetic as long as you wish.

"The next point in the *Précis* is the question of new operations in surgery. I shall show directly that many of



the operations which are performed now are based entirely on experiments on animals ; but I would like to point out that, from the ethical and moral point of view, it seems to me that this is an absolutely essential procedure. . . . There can be very little doubt that some of the anti-vivisection agitation, relating to so-called experiments in hospitals, actually bears upon this very point. The anti-vivisection party say that it is immoral to try a new operation, or a new drug, upon a human being in a hospital. They call it an experiment. But then they also say it is immoral to do it upon an animal. The only conclusion, of course, is that there should be no more new operations and no more new drugs used in the endeavour to relieve suffering or avert disease. It seems to me an absolute *impasse*. . . . The thing is either moral or immoral ; and I venture to suggest that any new operation, any new operative method or procedure, ought to be tried on an animal before it is tried on man."

Sir Victor Horsley was then examined, as follows, on the principles of antiseptic and aseptic surgery.

The Work of Lord Lister

Q. (*Chairman*) I am a little afraid of speaking of these scientific subjects which I do not understand, and of misrepresenting anybody ; but Dr. Bantock¹ generally seemed to think that surgery, as practised now, aseptically, was nothing but a return to the practice of any really careful surgeon before Lord Lister's views about antiseptic surgery were promulgated. What would you say with regard to that?—A. The answer to that, of course, is extremely simple, that in the practice of the so-called careful surgeon of forty or fifty years ago, there occurred numerous deaths

¹ For Dr. Bantock's evidence, see Chapter X.

from pyæmia, septicæmia, erysipelas, hospital gangrene, and so forth; and those do not occur now. Consequently, the practice of that careful surgeon was not modern aseptic surgery at all, and for a very good reason—he did not know his danger: he did not know that it was not merely sufficient for him to wash his hands; he did not know that he had to exclude every avenue of entry of microbes into the wound. It is wholly incorrect to make a statement of that sort.

Q. I think Dr. Bantock's view was that microbes did not cause the disease?—*A.* Oh, very well. . . . As I say, I began my surgical work in 1876, just after the introduction of the antiseptic system; consequently, I have seen the whole thing develop, and I affirm that our present practice is nothing but the logical outcome of Lister's teaching thirty-five years ago. That is to say, Lord Lister taught that you had to exclude the microbes from the wound; apparently they were in the air, and were ubiquitous; you had therefore to employ antiseptic substances to kill them as they fall into the wound; and that was the reason why he employed the spray for one thing, and antiseptic lotions for another—that you could not trust the condition of your instruments, and your dressings also, unless they were saturated with antiseptics. But Lord Lister himself said, over and over again, that inasmuch as these precautions were exceedingly irksome to the surgeon, he would be only too delighted to get rid of them; and no one was more delighted than he when it was shown that the risk of infection from the air was so slight that certainly the use of lotions was enough to avert it; and, therefore, away went the spray. . . . Of course, I have seen, as I said before, the absolute disappearance from my hospital, University College, of certain diseases; they have gone. Anybody who would be asked now to write an article on pyæmia or blood-poisoning, in a dictionary of surgery, could not do it; the diseases are

gone. And that is wholly due to the introduction and the proper execution of antiseptic surgery. Then, in regard to the saving of pain, the invention of antiseptic surgery has saved a degree of pain which is absolutely incalculable. I believe that it has saved more pain than anæsthetics. I suppose that most of us have been operated upon, at one time or another ; and one knows that the pain of a wound only lasts for about forty hours : and, with a wound that has been well made, it is a perfectly tolerable pain. After that, in the healing, there is no pain. But when I was a student, a simple case like the removal of a breast for cancer used to have painful dressings for six or eight weeks ; and amputations still more. If an amputation now is not healed in a week, you blame yourself, or the house surgeon. Fortunately, we very rarely do amputations now, owing to Lord Lister again ; very rarely indeed. But in the old days, amputations used to go on for a couple of months of very painful dressing.

Q. What is it that renders the dressing unnecessary ?—

A. The immediate healing ; the complete exclusion of microbes from the wound, and therefore, immediate healing of the tissues.

Q. The immediate healthy healing ?—*A.* The immediate healthy healing ; so that the saving of pain by the introduction of the antiseptic system, for which, of course, we also have to thank M. Pasteur, as well as Lord Lister, is enormous. Then, as a third point, the antiseptic system has led to the discovery of new operative treatment ; it has led to the discovery of absolutely new operations, by which conditions are now perfectly easily and safely treated by surgery which, before, when I was a student, were looked upon as hopeless.

Q. You connect antiseptic surgery with experiments on animals ?—*A.* Entirely, from beginning to end. Because,

you see, until you could show that a microbe had a toxic or destructive effect on animal tissues, there was no basis for antiseptic surgery; it was Pasteur's work, by experiments on animals, which proved that point; it was the crucial proof that was requisite. And the anti-vivisection society, in their opposition to antiseptic surgery, and their repeated quotation of Lawson Tait for instance, show, I think, a distinct lack of moral feeling. Anybody who could oppose the means of relieving humanity, and relieving the lower animals, by a means so simple in execution as antiseptic surgery, I think cannot be actuated by proper moral feeling. I have seen, over and over again, Mr. Lawson Tait's name mentioned in this connection. I am not here in any way to answer for Mr. Lawson Tait, who is dead now, of course: but I would like to point out his action in regard to the question of the institution of the Pasteur Institute in this country. Mr. Coleridge informed the Commission that no Pasteur Institute was established. If he means that no institute like the Pasteur Institute in Paris was established, then that statement is contrary to the fact; because the British Institute of Preventive Medicine, now called the Lister Institute, is a Pasteur Institute of the same class, established on the Embankment.

Q. When was it founded?—*A.* The Committee for its founding, I think, was instituted in 1891; at any rate, a meeting was held in 1893 in favour of it at Birmingham; and as I was very keenly interested in the subject from a public point of view, I went to that meeting. Mr. Lawson Tait attended that meeting; and the first resolution was, 'That the members of the medical profession in Birmingham and district cordially approve of the objects of the British Institute of Preventive Medicine.' Mr. Lawson Tait said that he fully assented to the resolution; feeling that, while he objected to a certain class of surgical investigations,

bacteriological experiments on animals had proved of great value." (See *Brit. Med. Journ.*, Feb. 11, 1893.)

Surgery of the Nervous System

The witness was next examined on the surgery of the nervous system. "I wish," he said, "to put to the Commission that there is not a single function of the nervous system, the principle of which we know now, which is not derived from experiments on animals. All our fundamental knowledge of the different parts of the nervous system is derived from experiments on animals. . . . Aristotle said that the function of the brain was to temper and soothe the heart, it was to cool the heart; and it was nothing but the experiments of Galen, simply opening the rigid box of the skull, and showing that the brain was as hot as the heart, that got rid of that idea. And then again, if you take the experiments of Bell on the nerve roots: the nerve roots of course were known to Galen, but their function was not dissociated until Bell did it, and then only in the case of one root. But his language is very striking. What he did was to expose the roots in a rabbit; and, as he had no electrical means of stimulation, he touched the two roots, first one with a point of a knife, and then the other, and he found that if he touched one there was no movement as the result, but if he touched the other, immediately there was a movement of the muscles—in other words, that the two things had different functions. This was, of course, without anæsthetics (1811). He says: 'I now saw the meaning of the double connection of the nerves with the spinal marrow.' Of course he did. The thing had been known to anatomy, certainly, for 1600 years; but it was not until an actual physiological experiment was done that the investigator now saw the difference between the two things. I always

think that is a most striking demonstration of the absolute value of experiment. And, as regards Bell's position, which has been referred to again and again in the evidence, the facts are extremely simple. Bell did a great many experiments. If you look at his original pamphlet of 1811, which is in the library of the Royal Society, it is very interesting; it is covered with manuscript notes of his experiments on the brain, the respiratory centres, and a great many other experiments on animals. He notes over a hundred that he did; and, as long as he was investigating the physiological side of nature, he made discoveries. He gave it up, and he then, later, became involved in a certain amount of controversy, because Magendie and the French physiologists, by their experiments, discovered the function of the other root—the sensory root. They proved that it was sensory by stimulating its central end, which is, again, the only way of discovering its function. And then Bell unfortunately entered on a priority discussion, and, in a good deal of feeling, said he did not owe anything to his experiments—I have forgotten the exact words—at any rate, he repudiated his experiments. As a matter of fact, at that time he was doing nothing, and was adding nothing to science. From the moment that he gave up experimenting on animals, he equally gave up his contributions to science. As I am speaking about Bell, may I just allude to a point about neuralgia? Namely, that it was Bell's experiments on the facial nerve, which is a motor nerve—as contrasted with his experiments on the fifth nerve, which also goes to the face, but is a sensory nerve—which saved people from having unnecessary operations performed on them for neuralgia. But it was not until he made this experiment on a donkey that the difference of function between these two nerves was understood at all. Anatomically they had been known, of course, for something like 1600 years."

After alluding to Marchi's method (see Dr. Head's evidence, Chapter IV), Sir Victor spoke of the surgery of the brain and spinal cord. "Of course, the surgical treatment of nervous disease includes operations on the brain, operations on the spinal cord, and operations on the nerves. In the case of operations on the brain, for instance, you realise that this is a question of being able to treat disease within a closed cavity. You cannot examine the part, you have to form your opinion as to what is going on entirely from your knowledge of the physiology of the brain; and that we owe, of course, in the greatest measure, to the discoveries of the late Professors Hitzig and Fritsch, and, in our country, Ferrier. That has all happened since 1870; and we are now able to cure epilepsy, we are able to cure abscess of the brain, and we are able to cure tumours of the brain.

"Then, in operations on the spinal cord, the same thing prevails. In fact, the first operation on the spinal cord I am responsible for, so that I know the history of that subject. The technique of that operation I owe entirely to experiments on animals, and I am very glad of the present opportunity to repeat what I have said on that subject. Then, as regards operations on the peripheral nerves, I have already referred to Bell's operative treatment of neuralgia as being guided entirely by his experiments on animals. Then we come to the great subject of nerve suture. Injuries to nerves now, of course, we deal with successfully by dividing the nerves again and sewing them together in various manners; and the initial work bearing upon that subject was carried out by Flourens, who was the first, to my knowledge, to make experiments on animals, to suture nerves together, to investigate their function."

Surgery of the Thyroid Gland

The witness then spoke of experiments on the alimentary canal, which had helped to advance the surgery of the stomach and intestines, especially the experiments of Lembert (1836), which had led to the improvement of intestinal suture. He then spoke of the surgery of the thyroid gland.¹ "Here again it is most interesting to see how anatomy misled, and in fact hindered, investigation; because even the great anatomist Luschka, as late, I think, as 1880, suggested that the thyroid gland really was of no service, and that it might have been put by the Creator in the neck for cosmological reasons, to pad it out, exactly like a bolster, for support. Of course he was unaware that Schiff, as long ago as 1859, had made experiments on the extirpation of the thyroid gland, and had shown that it was not an accidental structure, but that it was an organ which was necessary to life. Then, the rest of the history also of the subject is interesting, because it shows the necessity of experiments on animals for the direct scientific proof of a biological fact. Sir William Gull, and more especially Dr. Ord, had shown that certain patients exhibited symptoms resembling the cretins in Switzerland, and that this could come on them, as it were, as a disease; and Dr. Ord described that the thyroid gland was diseased in these cases. He had one autopsy. Then, Professor Kocher had noticed the same thing in his operations on the thyroid gland; but it was suggested universally, and especially in this country, that this was a disease of the sympathetic nervous system, and that if it had occurred in surgery of the neck, it was because the sympathetic nerve in the neck had been damaged during the operation. A Committee was appointed, and I was appointed to make the

¹ See also Professor Osler's evidence, Chapter IV.

crucial experiment of removing the thyroid gland in a monkey, in order to see whether that was the cause, or whether it was due to damage of the nerves in the neck : and my experiments proved that it was due to the loss of the thyroid gland. This was twenty-two years ago. I also suggested, in consequence of these results, that we ought to graft a healthy gland into people as a means of cure. I had no opportunity of doing that myself, but it was done abroad with apparent success. The result of these experiments, and of his own researches, led Dr. George Murray, of Newcastle, to suggest that we could compensate for the loss of the gland by injecting the juice of the gland, the secretion of the gland. And that was the beginning of the successful treatment of myxœdema and sporadic cretinism."

Later, Sir Victor Horsley described the incident of the German instrument-catalogue :

"In 1902, Mr. Coleridge issued to the public a letter, addressed as follows—it was addressed to ladies :

DEAR MADAM,

This catalogue, which I respectfully ask you to look at, has been translated from the German. In reproducing it thus, I have followed the immortal injunction, *Nothing extenuate, nor set down aught in malice*. I place it, therefore, in your hands, and leave it to exercise its influence upon your heart. If it lead you to feel that anything is better than that such things should be, if it lead you to know, beyond the reach of gainsay, that pitifulness is a higher thing in the sight of God than knowledge thus obtained, you will send me your help, great or little, according to your means, that I may do what I can,

as effectively as you make me able, to put an end to these unspeakable deeds, and I shall continue to be your and the poor animals' ever-faithful servant,

STEPHEN COLERIDGE.

"That letter is fixed into a catalogue, which is a translation of the German catalogue of an ordinary surgical-instrument maker, who makes physiological apparatus for holding animals. In that letter, nothing is said about these animals being fixed in this way, on tables and boards, under anæsthetics. There is not a word about the use of anæsthetics. I have had that catalogue sent to me, over and over again, by ladies, asking what this means. And it is all part of this constructive misstatement that experiments are done in England without anæsthetics. This particular instance, I think, is the most immoral, really, of any act of the anti-vivisection party; because the *Times*, in 1902, published a serious article upon it, drawing attention to it; and subsequently a correspondence arose, in which I took part, and in which we appealed to Lord Llangattock, the Duke of Portland, and several bishops who had supported the issue of this misleading document, to withdraw it. They have not done so.¹ And it does seem really a terrible condition of things, that persons holding such high authority should lend themselves to the issuing of a statement of that sort."

The witness then referred to certain statements made about him by Mr. Coleridge, which were not in accordance with facts. He then went on to suggest certain improvements in the administration of the Act.

¹ In July 1902 the Duke of Portland withdrew his name from the list of Vice-Presidents of Mr. Coleridge's Society (see *Zoophilist*, August 1902).

Rabies

Later, with regard to rabies, the following additional questions were put and answered :

Q. (Dr. Wilson) Of course, after what you have said, I need hardly ask you whether you have ever had any reason to doubt the efficacy of the Pasteurian treatment for hydrophobia?—*A.* Oh no, not the slightest, not since the Report of the English Committee.

Q. When they have to propagate the virus at the Pasteur Institute, do they propagate it from rabbit to rabbit?—*A.* Yes, from rabbit to rabbit.

Q. Do they use dogs at all now?—*A.* I do not know whether they occasionally do. But that is not the animal used for propagation of the virus; the animal used is the rabbit.

Q. From rabbit to rabbit?—*A.* From rabbit to rabbit entirely.

Q. So that there does not seem to be any need of introducing a new virus, as it were?—*A.* No.

Q. But the symptoms of rabies, as seen in the rabbit, bear no resemblance to rabies in the dog?—*A.* It is a very interesting thing; it all depends upon the rabbit. You may take twenty rabbits, as you know, if you have kept rabbits, and one or two will be quite playful, and even bite; and, when you inoculate those rabbits, a rabbit which is naturally excitable will bite as viciously as a rabid dog. And so will a guinea-pig. I have had a guinea-pig inoculated with rabies fly at me exactly the same as a rabid dog will. The majority of rodents simply become paralysed in the hind legs; they have the paralysed form of rabies, and not the excitable form. But it depends upon the animal.

Q. Then would you go so far as to say that the

Pasteurian treatment of hydrophobia has been so firmly established on experimental and logical lines that it can never be successfully discredited? Is that your strong feeling?—*A.* Certainly.

Q. That was my opinion at one time too; but I have rather doubted it since.—*A.* I have not the slightest doubt. I devoted a very long time to working out the accurate statistics at the time of the Government Commission; and, considering that the Pasteur system has reduced the death-rate from 15 to 0·3 per cent.,¹ it is such a margin of difference that I never can have doubt.

Q. Statistics can be made to prove or disprove almost anything. Of course, you know a great many patients—over 1200 cases have been enumerated—have died either during or immediately after treatment; I mean within the time stated by Pasteur.—*A.* Who collected these statistics? It was Dr. Luteaud. That evidence was certainly not received by any one who knew the facts. But there is the evidence which the English Committee collected for themselves, you know. They went over to France, and I myself was over there for two months, hunting up the cases which had been treated, and hunting up the authenticity of the rabidity of the dogs; and there is no doubt that the death-rate, then, that Pasteur obtained, was about 1·5, if I remember right. Since then it has been brought down to 0·3 per cent., which is a very usual death-rate of a preventive inoculation.

Q. And you believe that there is very little pain inflicted

¹ For this estimate of a mortality of 15 per cent. among bitten cases not treated by Pasteur's method, see Dr. C. J. Martin's evidence: *Minutes of Evidence*, vol. iii. p. 214. He gives a table of 3,127 cases of persons, who were bitten by dogs judged to have been suffering from rabies, but did not receive the preventive treatment. Of these 3,127 persons, 517 died of hydrophobia: that is 16·5 per cent.

on the rabbit by it?—A. Yes; I have seen rabbits completely paralysed in their hind legs, and beginning to be paralysed in their fore legs, eating and looking about, absolutely unmoved. It is a kind of painless paraplegia in the rabbit, evidently. Human beings only suffer, of course, when they get the spasm, and the apprehension of dying from an inevitably fatal disease, you know; that is the suffering of hydrophobia.

Experiments on Surgical Shock

A great part of Sir Victor Horsley's evidence was concerned with sixteen experiments on surgical shock made in his laboratory, in 1895, by Dr. Crile, now Professor of Surgery in Cleveland. "It is probably not recognised by the public," he said, "that, in getting rid of blood-poisoning, Lord Lister had left practically only one cause of death from operations. There is practically only one cause of death from operations, and that is shock, or collapse; it is the one thing we dread." In these experiments, injuries were inflicted on sixteen dogs, under anæsthesia, to imitate the conditions of shock, to find the best way of treating shock in human patients after surgical operations. Sir Victor Horsley explained that the phrases "not under full anæsthesia," and "incomplete anæsthesia," which occurred in the description of one of the experiments in England and two experiments in America, did not mean that the dogs were in pain. He was asked, "Then you are completely satisfied in your mind, that complete freedom from any kind of pain was enjoyed, if one can use the word, by the animals under the anæsthetic?" and he answered, "Certainly; exactly as I am certain that the patients whom I operate upon in the hospital and in my practice are free from pain." Either he, or Dr. Goodbody, one of the Assistant Professors at University College, had been present at every one of these

experiments. The animals were anæsthetised from beginning to end of the experiments, and were killed under the anæsthetic without recovering consciousness. The following questions were put and answered :

Q. (*Mr. Tomkinson*) All these experiments of Dr. Crile were for the same object ; to test the amount of shock caused by different operations, and the effect of it?—*A.* Yes, the effect of it.

Q. On the circulation of the blood?—*A.* Yes.

Q. And that was done sixteen times over?—*A.* Yes.

Q. Sixteen healthy, beautiful dogs—fox-terriers—were taken. To use an ordinary vulgar expression, do you think the game was worth the candle?—*A.* Yes, I am perfectly sure of it.

Q. You admit, do you not, that those operations, published as they have been in that form, have created a tremendous sensation, and probably one of the strongest sources of opposition to the system and attack upon it?—*A.* Yes ; because of the gross misrepresentations of those operations.

Q. But is not the description of them by the operator?—*A.* Yes, certainly ; but not the description that has been given of them to the public. On the contrary, several times they have been made the subject of pamphlets which I have been advised legally I could take no action upon, because in an ingenious way the fact of anæsthetics having been given was not introduced ; but the whole effect of the pamphlet was to convey to the ordinary man in the street that those operations were done without anæsthesia.

Q. Do you think so?—*A.* But the whole strength of the anti-vivisection party is the capacity for writing a thing and giving it a false impression altogether by means of innuendo. That is very glaringly shown in all this evidence which has been laid before the Commission.

Q. I cannot say that they have ever been described to me as having been without anæsthetics, but accompanied with an expression of opinion that complete anæsthesia was hardly possible through such tremendous operations, and was not likely to have been carefully supervised when the animals were kept alive the whole time?—*A.* Exactly. And on what authority has that last statement been made, that anæsthesia cannot be kept up under such tremendous operations? Why, operations that we do on human beings are far more tremendous by way of nerve-stimulation.

Q. I think there has been more than one witness here who has expressed great doubt whether a dog is easily kept just on the border line.—*A.* That is exactly the point that I referred to before. I felt sure that evidence of that kind had been given ; and I say that that is simply a matter of knowledge of giving anæsthetics. It is sheer ignorance to say that a dog cannot perfectly well be kept under chloroform, or any other dangerous anæsthetic that you like to mention.

VI

THE EXPERIMENTAL STUDY OF DRUGS

PROFESSOR CUSHNY, M.D., F.R.S., *February 26*
and 27, 1907

DR. A. R. CUSHNY, Professor of Pharmacology and Materia Medica at University College, London, said that he desired to explain the influence which the experimental method in pharmacology had exercised on therapeutics in the course of the last half-century, during which time the study of pharmacology had been carried on by the examination of animals. He took digitalis as an example. It was introduced about 1785, as a remedy for dropsy. It was used in dropsy, for many years, without any understanding how it acted. About 1860, Clarus said that digitalis slows and weakens the heart, and concluded that it was useful in aneurism, and in acute fevers. "That was the result of about seventy-five years of clinical observation. Within a few years, Traube and Brunton showed, by experimental methods, that one effect of digitalis is to raise the blood-pressure to a marked extent, which is exactly the worst treatment possible in aneurism." Brunton and Traube and Schneideberg showed that the heart was affected; but at that time the knowledge of the physiology of the heart was so imperfect that they could not go any further. It

was not until the physiology of the heart was developed that the exact way in which the heart was affected could be worked out. It was from these experiments that the exact indications for digitalis in cases of pneumonia had been made out.

Another instance of the value of experiment was the discovery that lead, administered as a drug, was useless to stop hæmorrhage. "Numberless examples of this increased accuracy in the therapeutic use of drugs might be cited as the results of the minute examination of their effects in animals. Many old drugs have been discarded as the result of these inquiries, while others have proved to have properties which were previously unsuspected. The physician now has a much clearer view of what his remedies can do." Professor Cushny went on to speak of the introduction of new drugs as the direct result of experimental inquiry. It was by this method that Liebreich had discovered the action of chloral, Baumann and Kast had discovered sulphonal, and Cervello had discovered paraldehyde. No soporific had been introduced in the last forty years, except by means of animal experiment. Again, it was by the help of experiments on animals that Koller had introduced cocaine into practice, and Vinci had introduced eucaine. No local anæsthetic had been discovered except by means of experiments on animals. Again, the whole group of antipyretics, such as antipyrine, antifebrine, and phenacetin, had been introduced by means of experiments on animals. Again, it was through experiments on animals that Fraser discovered the use of physostigmin in ophthalmic surgery, and the use of strophanthus in medicine. All the vascular-dilators, such as amyl nitrite, had been discovered by experiments on animals; so had all the vascular-contractors, such as adrenalin. Again, the diuretics, caffein and theobromin, and the urinary disinfectants,

including urotropin, had been introduced by the same method. Again, modifications, such as heroin and aspirin, of older remedies had been introduced by this method; so had the new antiseptics, such as lysol, thymol, and formaldehyde; so had ethyl chloride, apomorphine, and the preparations made from thyroid gland. "This list by no means exhausts the new drugs introduced by means of the experimental method in the last forty years, during which it has been systematically practised with a view to investigating the action of remedies. I have no desire to minimise the importance of other methods of investigation; but when one contrasts the number of valuable drugs introduced into therapeutics without the aid of experiments on animals, one finds it disappointingly meagre. I exclude the discovery of the local antiseptics, such as carbolic acid, iodoform, and salicylic acid. Apart from these, I find that in the last forty years, during which the experimental method has been so fruitful in valuable remedies, the only drug of even mediocre importance introduced by other methods is pilocarpin.

"Another service which the method of animal experiment has done to therapeutics is in the sifting-out of valueless drugs. A large number of old vegetable and animal bodies which used to cumber the pharmacopœias is slowly disappearing, as the medical profession learns that they are inactive, and as the theory on which they were introduced is shown to be erroneous. A much larger number of new bodies, the result of the activity of the chemical industry, have to be examined, and accepted or rejected as they prove to be useful or poisonous. And among those accepted, comparisons of their virtues have to be continuously made. So highly does the chemical industry value the aid given it in this way, that its leaders are no longer willing to wait for the dictum of

the university pharmacologists, as formerly, but have appointed pharmacologists and erected laboratories for animal experiment, at the cost of many thousands of pounds per annum. The duty of these pharmacologists is to examine the action of the new chemical products of the factory, to reject those which are useless or poisonous, and to suggest possible improvements in those which promise to be of value. In this way many hundreds of new bodies have been tested, many rejected, and some submitted to trial by the medical profession. The cruder drugs have been in many instances replaced by purer principles extracted from them; but these principles have all to be tested before they can be known to possess the virtues of the crude drug.

“This may be exemplified by the present position of ergot, a drug which has been in use for many centuries for its effect in causing contraction of the womb, and arresting hæmorrhage in labour. Ergot has always suffered from the fact that it is uncertain in action, some extracts appearing devoid of any influence on the uterus, while others have undoubted value. Attempts have been made for many years to obtain a more satisfactory body by isolating the active principle of ergot. Within the last year a German chemist has put on the market an apparently pure substance, clavin, which he supposes to be the essential factor in ergot. This view may be tested in two ways. Clavin may be injected into women in labour who show signs of hæmorrhage, and in course of years doubtless the question as to whether the clavin is a valuable addition to therapeutics, an inert body, or a poison, may be determined. On the other hand, the effect of ergot on the uterus of animals is quite well known, and half a dozen experiments on anæsthetised animals would suffice to settle the question. As a matter

of fact, clavin has proved quite inert in three experiments in which I have tested it.

“Another direction in which animal experiments have proved of the greatest importance, is in discovering and testing remedies to be employed in cases of poisoning. Poisoning occurs so rarely that in practice it is impossible to test the value of antidotes; and even when a patient recovers after the giving of an antidote, the question always arises whether the quantity of poison taken was really a fatal dose, whether vomiting or slow absorption or some other factor was not really responsible for the recovery. In animals, on the other hand, the exact dose required to kill may be ascertained, and the effect of antidotes may then be examined with accuracy. In this way, many supposed antidotal measures have been shown to be valueless, or worse, through the waste of time which their administration involves.”

Professor Cushny went on to speak of the standardising of drugs by animal experimentation. “This method consists in finding the smallest dose of the preparation which induces a certain result in animals. For example, digitalis is assayed by finding the smallest quantity required to arrest the heart of the frog in a given time. If the doses of two preparations vary as 1:2 for the frog, they will also vary as 1:2 in man. The activity of strophanthus and squills is determined in the same way as that of digitalis; that of ergot is found by experiments on cats, rabbits, and dogs; cannabis indica is assayed on the dog, which reacts much more exactly than the cat or rabbit; and adrenalin and products of the suprarenal gland also are assayed on the dog. The importance of standardisation of these drugs can be easily understood; and the ordinary pharmacist is quite helpless in the matter. For example, I have found that of two tinctures of digitalis supplied

me by a perfectly reliable firm, the one was four times as strong as the other. If a patient were treated with the weaker for some time in the increasing doses which would be necessary to elicit the therapeutic effect, and then the treatment were inadvertently continued with the stronger in the same dose, the results might very easily be disastrous. In the case of ergot, the conditions are even worse; for much of this drug on the market is practically inert, and the preparations, of course, valueless; a physician may thus depend in an emergency on the action of a drug which is without action. I may remind the members of the Commission that ergot is very largely used in cases of hæmorrhage in child-birth, in which it is absolutely essential to lose no time, and to be certain of one's remedies, if the woman's life is to be saved. In regard to another drug, *cannabis indica*, I have personal knowledge that 20,000 pounds of it were offered for sale to a firm, which, before closing the deal, had a sample tested on animals, and, finding it inactive, refused the consignment. This firm informs me that since the introduction of physiological assay, 'we have had practically no complaints whatever as to the inefficiency of products of *cannabis indica*, quite a contrast to our experience previous to this time.' "

Professor Cushny was asked by Sir William Collins whether Koller had not discovered the properties of cocaine by experiment on himself, and whether Sir Benjamin Ward Richardson had not studied nitrite of amyl on himself. Asked about the effects of hemlock, he said that it was, so far as he knew, universally a poison to vertebrate animals. He was quite satisfied that no animal suffered pain under his experiments. He had very frequently attended experiments by other persons, and had never seen anything which could have caused needless suffering to the animal. Asked to suggest any alteration

in the present system of granting licences, he said that his only criticism was that the terms of a certificate were sometimes too narrowly defined. Asked whether experimental studies tended to make a student neglect bedside observation, he said that they had the opposite result; they helped him to be more careful in his bedside observations. He was then asked about local anæsthesia, and explained that abdominal operations were frequently done under local anæsthetics, because the abdominal organs had really very little ordinary sensation of pain. He was then asked about general anæsthetics, and about curare (see Chapter IX).

He was again asked about the testing of drugs on animals. He said that every drug, or almost every drug, that had been widely recognised in medicine had been investigated, and had had a certain number of experiments performed with it in the last 30 to 35 years. Such drugs were generally tested before they were put on the market by the more reputable firms, and then they had to run the gauntlet of a second series of investigations by more impartial investigators in the University laboratories. A certain number of these experiments caused some pain; and in a certain number the animal died. Asked about experiments made by man on himself, he answered: "I think that almost every man who is going to introduce a drug, first, after investigating its action on rabbits, dogs, frogs, and so on, tries it on himself, before trying it on a patient; if he finds it acts upon himself, he goes on to use it in medicine. There have been some examples of drugs being introduced not through animal experimentation, but through experiments on man, which have rather discouraged the rest of us from following that method. The one which I have given already was Christison, who ate the Calabar beans without any previous experience,

and almost died of it. Another was Koeppe, who tried the effects of digitoxin on himself, and was despaired of for 24 hours."

PROFESSOR DIXON, M.D., *December 4, 1907*

Dr. W. E. Dixon, Professor of Materia Medica and Pharmacology at King's College, London, gave evidence as representative of the Therapeutical Section of the Royal Society of Medicine. The earlier part of his evidence was concerned with anæsthetics and with curare (see Chapter IX). He then went on to speak of the difficulty of acquiring knowledge by clinical observation alone, and gave the following instance: "I happened to be on a Committee of the British Medical Association for suggesting what drugs shall go in the new British Pharmacopœia, when it became necessary to find out the value of certain new silver compounds. Accordingly we sent out notices—I think we sent out three hundred notices—to the leading ophthalmic surgeons, and surgeons attached to Lock hospitals, asking them what was the value of these new compounds: and the results which came in were the most remarkable I have ever seen. It would be thought that in dealing with the action of silver compounds used for eye work, conjunctivitis, and various forms of inflammation of the eye—that if it is possible to make any definite statement from clinical observations, it would be about a disease which you can actually see; you can apply the drug and say at once whether it has any value or no value. And yet the decisions of these expert physicians and surgeons as to the value of these new drugs were exactly divided. Some of them said, 'This drug is our sheet-anchor in the treatment of disease.' Others regarded the same drug as quite inert and worthless."

Professor Dixon was then asked about the standardising of drugs. "There are five drugs," he said, "digitalis, strophanthus, squill, ergot, and cannabis indica, the chemistry of which we know so little that we cannot standardise them in any way other than physiologically. Of these, two drugs, namely digitalis and ergot, are of the most important in the pharmacopœia. There are ten different ergots there (*handling in a list*). Those ergots were bought at different chemists' shops. This is simply to show the variation in the action of ergot. A practitioner, say, having to treat a case of a woman in labour, gives one of those ergots, and probably, in three cases out of four, it is inactive. The same applies to digitalis." To avoid that risk, such drugs must be standardised, and this could only be done by experiments on animals.

Asked about a statement that pigs are absolutely insensible to morphia, he answered: "How was the morphia administered? There are all sorts of statements of that kind made: one is continually coming across them. For instance, they say henbane is not poisonous to hogs, and it is to hens: but that is not true, of course. It is poisonous equally to both of them if you give it in the correct way. Some drugs are given by the mouth, and are not absorbed, and people say they are not poisonous." Given a chemical substance which was a poison to any animal, it would be poisonous to any other animal if it were properly got into its blood.

Professor Dixon went on to speak of certain researches made in his own laboratory on lymphocytosis, arteriosclerosis, the action of tobacco-smoke and the origin of nicotin-immunity, the action of new cardiac tonics, and the action of caffein, urea, and saccharine on the kidneys, and the relative action of new local anæsthetics.

He was then asked again about the assaying of drugs on

animals. He said it was his belief that many hundreds of patients die annually, from digitalis and its allies not possessing the virtues which are required of them. It ought to be possible to prevent the sale of such drugs unless they were standardised. Digitalis, strophanthus, and squill were standardised on frogs; ergot on cats, the operation being done under an anæsthetic, and the animal killed before recovering consciousness; cannabis indica on dogs.

Asked as to any difficulties or annoyances caused to workers by the present Act, he alluded to the long delay which often attended the granting of a licence. Asked whether he would approve of the performance of experiments on animals, under a lethal dose of anæsthetic, to gain manual dexterity, he answered, "Yes; I think it would be a wise provision to make for the sake of humanity." Asked whether he would agree to the admission into laboratories of independent, properly accredited witnesses, to reassure the public, he said that he would be the last person to object; he would not object at all. Asked whether he would approve the General Medical Council being held responsible for the issuing of licences rather than the Home Office, he said that he thought it would be a very good plan. Asked about the general value of the Act, he thought the medical profession would be strongly against the Act being repealed, because any medical student, any irresponsible person, could then make all kinds of experiments at his private house. He thought the Act was of advantage alike to the animals, and to the public, and to the men working under the Act. Asked about the inspectors' visits, he said: "The inspector, as it happens, has come in several times just when I have been doing an experiment. He has come in twice to my knowledge when I have been giving a demonstration—

in the middle of a demonstration to the men. He always examines the animal with interest, and he waits until the demonstration is over, observes the result of the demonstration, and then examines the animal, and sees it killed. Those are exceptional occasions, because he does not usually hit off the time when there is an experiment on. Under ordinary conditions he comes and perhaps asks to see the laboratory book, and asks to see the experiments going on. He sees every worker in the laboratory if he is about. He inquires after the animals, sees every animal under experiment, and sees the condition of the cages in which they are kept; and he would, I presume, make objection if they were not properly housed, and so on."

VII

TROPICAL DISEASES

DR. C. J. MARTIN, M.B., D.Sc., F.R.S., *July 10 and 17, 1907*

DR. MARTIN, Director of the Lister Institute, Chairman of the Anti-Typhoid Inoculation Committee appointed by the Secretary of State for War, Member of the Plague Committee, and Member of the Tropical Diseases Committee of the Royal Society, gave evidence on behalf of Prof. Starling's Committee. (See also Chap. IV.)

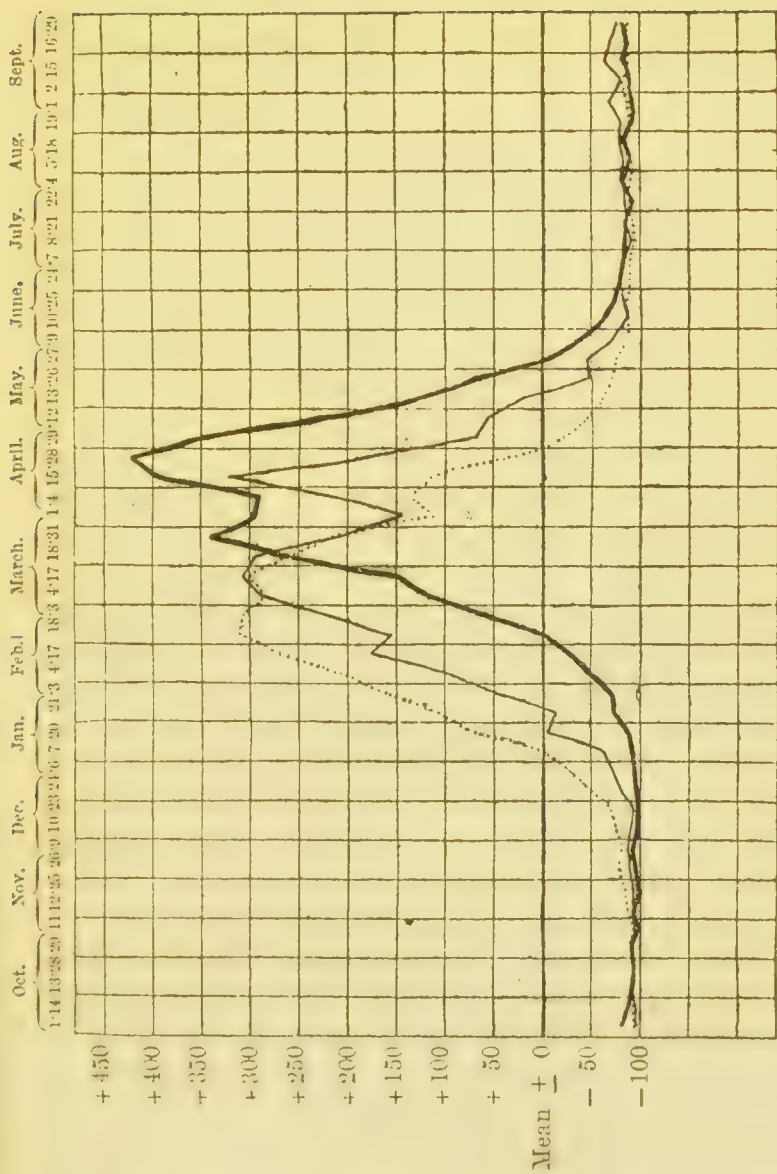
Plague

Asked by the Commissioners to give an account of plague, he answered that plague was a disease of great antiquity. The constant association of deaths of rats and mice with outbreaks of plague among human beings had been observed for ages, but was not understood till Yersin and Kitasato, in 1894, found the germs of the disease, grew them on artificial media, and reproduced the disease in animals by inoculation with these pure cultures. "Once the cause of the disease was known, it was easy to determine that the disease from which rats and mice simultaneously suffered was also plague. All the conditions, favourable and unfavourable to the existence of the infective agent, could now be studied; and the

means, whereby the infection leaves the bodies of persons or animals dead of the disease, could be determined. The relationship in time and space between human outbreaks and disease in rodents could be worked out, and the influence of epidemics amongst rats, in the spread of the disease amongst human beings, determined. The relationship of rat plague to human plague has been found to be of such a character as (taking into consideration the small extent to which bubonic plague is infectious from man to man) to indicate in the strongest manner that the spread of the disease in man is conditioned almost entirely by the occurrence of plague amongst rats in his vicinity. The Commission at present investigating the question of the spread of plague in India have, by extensive experiments on animals, succeeded in showing that the infection is carried from rat to rat by the agency of the fleas infesting these animals. By the same agency they have been able to produce epidemics amongst other animals, guinea-pigs and monkeys; whereas all other means failed to give rise to the epidemic spread of the disease. The Indian rat flea also feeds upon man when his natural prey is not available: so that a possible, and indeed probable, means whereby the infection is carried from rats suffering from the disease to mankind has been

The accompanying chart is reproduced from the pamphlet "Plague in India, Past and Present," by Colonel Bannerman, I.M.S., Director of the Bombay Bacteriological Laboratory. The pamphlet is published by the Research Defence Society.

The chart shows the number of plague-infected rats found among the hundreds daily caught and destroyed in the city of Bombay during a time of plague, and the number of human deaths from plague during the same period. The dotted line shows the number of brown rats (*M. decumanus*) found infected; the thin line shows the number of black rats (*M. rattus*) found infected; the thick line shows the number of human deaths from plague.



RATS AND PLAGUE.

established. Further experiments carried out in houses or huts, where many cases of plague occurred, have shown that animals (guinea-pigs or monkeys) are attacked by the disease when placed in such infected huts without protection from fleas; but if the simplest means are adopted to prevent the fleas from reaching the animals, by a gauze covering, or a layer of sticky paper wider than a flea can jump, or by suspending the cage at a distance from the floor greater than a flea can leap, they have in no case contracted plague. . . . It need hardly be insisted upon that the complete comprehension of the spread of the disease under natural conditions is essential to successfully devise preventive measures. Without such knowledge our efforts will, in all probability, be misdirected. A knowledge of the causation of plague has enabled a useful method of protective inoculation to be devised. Efforts towards the production of a curative serum have unfortunately, so far, been disappointing, as the means employed have not produced an anti-serum of sufficient potency."

Asked about Haffkine's preventive treatment, he referred to Pasteur's discovery that animals could be protected against chicken cholera and anthrax by inoculation with attenuated cultures of the organisms which cause the diseases; and to Gamaleia's discovery, in 1888, that it was not essential, for the production of protection, to use living germs, since some degree of immunity could be induced by the use of the chemical products contained in dead germs. This discovery made by Gamaleia had been extended to the protective treatment against cholera, typhoid, and plague. "In 1896, plague broke out in Bombay, and spread throughout the city, and extended to neighbouring places. The general sanitary measures employed proved powerless to prevent the spread of the disease.

Haffkine therefore turned his attention to the problem of preventive inoculation against plague, in the hope of arriving at a useful method which could be applied upon a large scale to man. After numerous experiments with cultures of the plague bacillus, which had been killed by a variety of means, Haffkine decided upon a broth culture of the organism, sterilised by heating to 65°C . Having tested the vaccine by inoculating first himself and then the *personnel* of his laboratory, the first extensive trial of the efficacy of this vaccine was made in January, 1897, upon the inmates of the Byculla Jail, where plague had broken out. The submission to inoculation was voluntary. The jail contained 337 adults of both sexes, who were not obviously infected with plague; 154 offered themselves for inoculation. Three of these were, however, in the incubation stage of plague, for they developed plague the same evening. For the purpose of arriving at an estimate of the value of the preventive inoculation, these persons in the incubating stage of the disease must be eliminated from the calculation, as also many cases amongst the uninoculated, who also developed plague upon the day the injections were made; leaving 177 uninoculated and 151 inoculated. The result of this experiment was as follows:

	Number of Persons.	Cases of Plague.	Deaths.
Uninoculated . .	177	12	6
Inoculated . .	151	2	0

“In December, 1897, an outbreak of plague occurred in another jail in Bombay (Umkardi), thus affording a further opportunity of testing the efficacy of Haffkine's

vaccine, under conditions as favourable for the formation of a judgment as it is possible to obtain in connection with human diseases.

	Number of Persons.	Cases of Plague.	Deaths.
Uninoculated . . .	127	10	6
Inoculated . . .	147	3	0

“Another opportunity for testing the efficiency of Haffkine’s vaccine occurred in the village of Undhera. The whole population of this village was assembled in family groups, and the intention was to inoculate every alternate person. The total number of inoculated persons was 513, and uninoculated 437. The larger number of inoculated was owing to the insistence of some individuals to participate in the possible advantage of inoculation. The epidemic lasted forty-two days, and the result was as follows :

	Number of Persons.	Deaths.
Uninoculated	437	26
Inoculated	513	3

“On the strength of the favourable results of the above experiments, Haffkine’s method of inoculation was officially adopted by the Government of India, and arrangements were made for the manufacture of vaccine on a large scale.

“During the plague season 1902-3, 505,398 persons were inoculated in the Punjab alone. The official report gives details concerning the villages of 12 districts in which the proportion of inoculated persons was greatest. The

number of inoculated in these 12 districts was 224,428, and the uninoculated, as estimated from the total population, 639,600. The number of cases and deaths from plague were as follows :

	Number of Persons.	Cases.	Deaths.	Cases per cent.	Case-mortality per cent.
Inoculated . .	224,428	3,393	814	1·5	23·9
Uninoculated .	639,600	49,430	29,623	7·7	60·1

“ In the case of the district of Amritsar, the inoculations and subsequent observations were all made by one individual ; and, as these show evidence of having been conducted with particular care, they are worth quoting :

	Number of Persons.	Cases.	Deaths.	Cases per cent.	Case-mortality per cent.
Inoculated . .	30,839	572	150	1·21	26·2
Uninoculated .	130,927	11,233	7,888	8·58	70·2

“ The value of inoculation against plague is undoubted. According to the less favourable of the above figures, the chances of a subsequent infection are reduced to one-fifth by this means, and the chances of recovery are two and a half times as great as in the case of the uninoculated. In other words, the mortality has been reduced to one-twelfth by inoculation.”

Plague, said Dr. Martin, disappeared from our own country, for reasons which we do not understand in the least. In Egypt, there had been several small epidemics : he could not say whether Haffkine's vaccine had been used or not. In the eastern dependencies of France, Yersin's

serum was used to some extent: Lustig's serum had also been advocated. Unfortunately, once a patient was stricken with plague, a serum treatment was not of great value. Yersin's serum was very weak compared to the sera against diphtheria and tetanus; and to that extent it was a disappointment.

The following questions were put and answered:

(*Sir Mackenzie Chalmers*). You yourself have been on this Plague Commission, I think?—Yes.

And you have been in India?—Yes.

And were working there. What is the number of persons on the Plague Commission?—I think there are eight doctors.

How many assistants?—I think, taking the minor assistants, it must amount to one hundred.

Are all those people continually exposed to the virus of plague?—Yes, more or less.

What precautions have been taken to secure them, if any?—They were inoculated at regular intervals.

With Haffkine's prophylactic?—With Haffkine's prophylactic.

Were they inoculated once a year?—They were inoculated more frequently than that, I think.

Among your eight doctors who are continually handling this virulent material, how many cases have you had of accident?—None amongst the doctors.

Yet, from the nature of your experiments, I suppose you are continually in contact with the poison?—Yes. They take reasonable precautions so as not to get it inside themselves.

One of them, at any rate, has had some abortive attacks of plague, has he not?—One of them suffered from what is called *pestis minor*: I did not mention that.

However, no effects followed. The prophylactic was effective?—Yes.

How far was it effective among the native assistants?—To my knowledge there have been three cases of plague amongst them.

Fatal or non-fatal?—They were non-fatal.

Therefore, amongst several hundred people in continual contact with this disease, the Haffkine vaccine has been a successful prophylactic?—I can only say that they did not die of plague.

And they had every opportunity?—How far that is to be attributed to Haffkine's prophylactic can only be found out by dealing with a larger number of figures.

The statistics are too small?—They are too small for me to attach importance to.

Still, from your own experience, do you think it is right, or superfluous, to inoculate, with Haffkine's vaccine, these people specially exposed to the plague?—I can best answer that by saying that I have been always careful to inoculate myself.

More than once?—Yes.

It produces, I suppose, for two or three days, a great discomfort?—Not at all. In my case, it produced no discomfort, because I went about it in a gradual way, taking a very small dose in the first instance, and the next week a little more, and so on, which is the best way to immunise anybody.

Just as you would a horse?—Yes, just as you would a horse. The dose was small to begin with, and gradually multiplied. I was never in the least incapacitated, either in the way of appetite or my ordinary occupation. I had a slight soreness.

When a strong prophylactic dose is given, there is generally, for two days, discomfort, is there not?—Yes, but it is very seldom that it would incapacitate a European from going about his ordinary business. Occasionally it happens that a man, for some reason or other, is distinctly invalided, say, for thirty hours: but that is unusual.

It does not take place if the inoculation is carried out gradually?—No, it does not take place then.

Your experiments in India with plague have been animal experiments, of course?—The experiments have been entirely on animals.

And you have, as the result of those experiments,

discovered at any rate the common mode of transmission of plague?—Yes, I think so.

Namely, by the rat flea, as you say?—Yes.

You would hardly say that that is the only mode?—It is not the only possible mode: but the evidence adduced by the Commission is such as to lead one to regard it as the only serious mode of epidemic spread.

Therefore, at any rate, you have accomplished this by your experiments, that you know the evil to be dealt with?—Yes.

Though you have not yet succeeded in discovering anything like a remedy after the onset of the disease?—No, not yet. Whether it is possible to satisfactorily interfere is another question.

Still, is it, or is it not, an important step to know the means by which a disease is communicated from one person to another?—I think it is an essential step.

And that, you say, cannot be obtained without the use of animals for experiment?—No.

Incidentally, if the rat flea is the ordinary way of communicating the disease, how is it that doctors, continually handling these animals in transferring the fleas from one to another, were not bitten?—I imagine they were occasionally bitten. For one thing, they wore top boots.

But in combing out the fleas, to take them from one animal to another, I mean?—The animal was chloroformed and the fleas were chloroformed, otherwise it would have been impossible to have dealt with them; the Commission would have been exterminated!

But you think there must have been occasional bites; and do you attribute the immunity to the prophylactic or not?—I attribute some of the immunity to the prophylactic.

(*Lord Selby*). Why did you say the flea was chloroformed?—The animal and the flea. If they were getting fleas off the animal, they would put the animal in a receptacle with chloroform vapour; and that quiescent sent the animal to sleep, and the fleas to sleep, and one could then pick them off at one's leisure.

(*Sir M. Chalmers*). And then apply them to another animal?—The fleas recovered from the anæsthetic in a few minutes, and they could then be dealt with as wished.

Is it known at all how a flea, biting either an animal or a human being, can deposit the plague bacillus?—It is known that the flea, whilst biting, commonly discharges from behind.

In the process of biting?—Nearly all suctorial animals, whilst sucking blood at one end, discharge the contents of the intestine at the other, and in that way they can discharge infection.

Then the discharge of the intestine gets on to the puncture?—Yes.

And that is the mode of entry?—That is the probable mode. It itches, and you rub it.

It is exactly the same with an animal?—Yes, the same with an animal.

Dr. Martin further stated that, with Haffkine's vaccine, there was certainly some immunity left in animals, and also apparently in man, after twelve months. The Plague Commission was still at work, but was resting during the hot weather. The previous Plague Commission, which reported long ago, was a Commission to take evidence: the present Commission was essentially a working Commission. The previous Commission had formed an opinion from the early records of inoculation a long time ago. Asked about the Malkowal disaster, due to the contamination of a bottle of the plague vaccine with the germs of tetanus, he answered that the Lister Institute had reported, "That the probabilities are in favour of the view that the tetanus impurity was primarily in the fluid, but that they did not feel justified in asserting this as a proved fact, in view of the possibility of contamination at Malkowal." Asked whether he thought that the preventive treatment was having a valuable influence in checking the spread of plague, he answered, "Very valuable." Finally, he gave

the instance of the sweepers in the city of Bombay during the plague. "The sweepers were being utilised, in connection with our Commission, to gather in dead rats every morning, and the amount came to upwards of 5,000 dead rats a week, all of which were examined for plague by the Commission. These men were under the control of the health authorities, the municipal officers of health: they were working to assist us. The figures there, I think, are large enough to be significant. There were 7,182 inoculated sweepers, and there were 418 uninoculated sweepers. The attacks of plague were as follows: Amongst the 7,000 inoculated, 14; amongst the 400 uninoculated, 28. The deaths among the 7,000 inoculated were 13, and among the 400 uninoculated, 26. In other words, the percentage of attacks among the uninoculated was 6·7, and among the inoculated, 1·9."

Dysentery

"Epidemic dysentery," said Dr. Martin, "is an infectious disease, which is more particularly liable to assume serious proportions when individuals are crowded together in jails, asylums, or camps. It is in war, especially, that dysentery becomes a formidable disease, both on account of the high death-rate and of the number of men thrown out of action, often for prolonged periods. Knaak states that in the American War of Secession, 1861-4, there occurred 259,071 cases of acute dysentery, with 5,576 deaths; and of chronic dysentery, 28,451 cases, with 3,855 deaths. In the Franco-Prussian War of 1870-1 there were 38,652 cases (49 per 1,000) and of these 2,380 died. The bacillus of bacterial dysentery was first isolated by Shiga, in Japan (1898). Kruse found the same bacillus in an outbreak of dysentery in South Germany, and Flexner in Manila. The bacillus is pathogenic for the guinea-pig and rabbit. In these

animals the bacillus itself, or the poison obtained by growing it in broth, produces dysenteric diarrhœa, severe nervous symptoms, paralysis, and death within a few hours or days, according to dose. In the intestines of these animals, morbid changes, similar to those occurring in man, have been observed.

“Shiga, in 1901, first immunised horses against the dysentery bacillus, and employed the serum as a therapeutic agent in man. Kruse obtained a similar serum by using the bacillus isolated by him in Germany; and Rosenthal, Todd, and others have added to the knowledge of the subject. Anti-dysentery serum possesses preventive and curative properties when employed on animals inoculated either with the living bacillus or its toxins. All the pathogenic effects referred to as occurring in experimental animals can be prevented or cured.

“No extensive data as to the value of anti-dysentery serum as a therapeutic agent in man are as yet available; but, so far as it has been used, the results recorded have been of a very promising character. Shiga used his serum on 298 cases, and obtained a reduction in the death-rate to one-third of the rate in cases not treated by the serum: the death-rate in cases receiving only serum-treatment was 10·8 per cent.; and in 2,599 cases treated by other means, 35·4 per cent. Kruse treated 100 cases with good results: of 19 patients under 10 years one died, which is equal to a death-rate of 5 per cent., the usual mortality in cases of this age being from 13 per cent. to 20 per cent. The injection of the serum was followed by a rapid reduction in the number of stools, and by improvement in the general condition. Similar results were obtained at Moscow by Rosenthal, in 157 cases treated with serum. The death-rate was 4·5 per cent., compared with 10·11 per cent. in other hospitals during the same epidemic. When the serum was

used in the first three days, recovery took place in one or two days. Rosenthal's serum was employed in the Russo-Japanese War. Koreutchewsky treated 70 cases, and found the treatment very efficacious: the painful symptoms were controlled as by a narcotic. Barikan treated 59 cases in Manchuria, with one death. Vaillard and Dopler treated 96 cases, all of a severe type, with one death."

Dr. Martin added that, from the experiments on animals, he regarded the dysentery antitoxin as, for practical purposes, coming after the diphtheria antitoxin. "In practical value it will, I think, make a good second to the diphtheria antitoxin." But it had not been so extensively tested; and it had not been used in India to any extent.

SIR DAVID BRUCE, K.C.B., F.R.S., *November 5, 1907*

Colonel Sir David Bruce, R.A.M.C., Member of the Advisory Board of the Army Medical Service, Member of the Sleeping Sickness Commission, and Chairman of the Mediterranean Fever Commission, stated that he had worked in Malta during 1884-9, at Netley during 1889-1904, in South Africa and Zululand during 1894-7, in South Africa through the war, in Uganda in 1903, and then in Malta again. He was asked, "You have had very large experience, spread over a good many years, in animal experiments; what is your opinion, generally, on the subject?" He answered, "My opinion is, that it is the most valuable method we have of advancing medical knowledge, and without it we could hardly advance a single step safely. These questions are so complicated, and one is so apt to fall into error, that unless each step is checked by animal experimentation one soon goes wrong." He had especially worked at Malta fever, nagana, and sleeping sickness.

Malta Fever

"You can put down the average duration of this disease at the extraordinarily long time of four months, or 120 days. Of course, it lasts much longer in some cases. Many are invalided from the service altogether, and a certain percentage of them die. The disease is characterised by extreme weakness; that is to say, a man is brought down to the very edge of the grave without absolutely dying; he is extremely emaciated; he suffers from severe pains in the joints and nerves; and altogether we look upon it as a more severe disease than even enteric fever, although the mortality in the case of Malta fever is not nearly so large as in enteric. It is found all over the world; you may say it is a world-widespread disease in the sub-tropical and tropical regions of the earth. It was called Maltese fever because it was first studied there. It has other names; for example, rock fever in Gibraltar. In Kimberley it is known as camp fever; and every place gives it its own name. In the nomenclature of diseases it is known as Mediterranean fever, with the synonym of Malta fever. The change from Malta fever to Mediterranean fever was a little politeness to the Maltese people; but I think it is better to keep to the term Malta fever until they do something to clear their island of it."

Sir David Bruce handed in charts and tables showing the incidence of the fever among the troops in Malta at different times in the year. He pointed out that the incidence among the officers was about four times as great as among the men. He handed in a table showing the number of officers, non-commissioned officers, and men invalided home. He then proceeded to give an account of his discovery of the way in which the disease was conveyed:

"It was laid down years ago by Professor Koch, the

famous German bacteriologist, that the proof that a micro-organism is the cause of a certain disease must fulfil three conditions. (1) You must find the same micro-organism in every case of the disease; (2) you must be able to cultivate that micro-organism outside the body in such a way as to remove it from all products of the body; and (3) you must be able, by the injection of this artificial growth into healthy animals, to give rise to the same disease. Certainly one of the most important things, in beginning the study of a bacterial disease, is to be able to put your finger on one germ, and say *that* absolutely is the cause of the disease. Having settled that, you can go on to study its natural history further. In the case of Malta fever, another animal besides man is susceptible to the disease under ordinary natural conditions; and that animal is the monkey. So that, after having tried the ordinary laboratory animals, such as the rat, guinea-pig, rabbit, and finding no result from them, I tried monkeys. When one injects this micro-organism into the former animals, there is no rise of temperature, and no appearance of disease; but in trying it on monkeys, one found that they took a fever very similar to the fever in man, that they suffered from arthritic pains, and that, so far as you could see, they suffered in the same way as man did; and lastly, when they died, the same micro-organism was found in large numbers in pure culture in their organs. In this way Koch's three postulates were satisfied, and it was proved that the *micrococcus melitensis* is the cause of Malta fever. That was in 1886. I found this particular micro-organism in every case of death from Malta fever in man. After death, at the *post mortem*, one removed a small quantity of the spleen, and planted it out in various materials suitable for the growth of these things; and on a particular material, and at a particular temperature, this particular micrococcus grew, and

it was from the growths, of course, on the artificial media, that the animals were inoculated."

"Of course, later on, when we had the Commission, all these experiments were done in large numbers. I do not know how many monkeys were used as experimental animals, but there must have been some hundred and fifty or two hundred monkeys; we had a large number of monkeys, and in using them they were used in many ways. That was in 1904-6. For example—in finding out how the *micrococcus melitensis* leaves the body, which is naturally a very important thing to find out—it is a minute germ, and it might possibly come out in the expired air, in the urine, or it might come out, as the bacillus of enteric fever principally does, from the alimentary canal, in the dejecta. Then, of course, in examining how the micrococcus leaves the body, naturally you would examine the sweat, the saliva, the tears, and the other secretions. At the time this work began, everybody supposed that Malta fever was due to effluvia, that it was due to this minute micro-organism finding its way into the air of wards from the sick, or into the air of rooms from drains, sewers, etc., and so causing infection. Now, to put that to proof, you have to drive thousands of cubic feet of air through an apparatus, in order to collect the micro-organisms contained in the air; and, as you collect a number of ordinary air-bacteria, you could not, by simple examination under the microscope, distinguish between the *micrococcus melitensis* and these others. So that here it is necessary to use an animal. The animal, as it were, picks out the pathogenic organism which alone has the power of growing in the tissues and so causing the disease. All the other common organisms found in the air are killed off by the tissues and fluids of the animal, leaving the particular pathogenic one alive: and so its presence in the air can be proved. The micro-organisms would be

taken from the air into a quantity of water ; that water would be centrifuged in order to throw the solid matter down ; and that solid matter in a small quantity with water would be injected under the skin of a live animal. In this particular case, as the monkey was the only susceptible animal, the Commission used monkeys. At one time it was thought, for example, that the Grand Harbour of Malta was the breeding-place of this fever. On account of its being used as a sewer for hundreds of years, it was until recently in a disgraceful condition, and it was considered that the sailors got the fever when going back to their ships at night. Now, in order to prove that, a large quantity of water is passed through a Chamberlain filter, in order to retain the little particles contained in the water ; and then, removing these germs from the filter, you can experiment as to whether they contain the *micrococcus melitensis*, by injecting them under the skin of a monkey. If there are any *micrococcus melitensis*, they will probably set up the disease, and the one particular germ will be recovered from the animal afterwards, all the other harmless ones having been killed off by the tissues of the animal. It was found, from these experiments, that the micrococcus was not conveyed either by the air, or by the effluvia, or from the harbour water ; nor from dust collected in suspicious places where one might expect to find it, contaminated places ; nor from the air and dust, for instance, of fever wards, or of rooms where cases of the fever had occurred, and so on. All those experiments were negative.

“It was discovered, by animal experiment, that infection could be conveyed by the mouth. This discovery came, as it were, as an accident, at the end. In an investigation of this kind, you naturally examine all the animals round about, in order to find out if any of them harbour the disease. The goat was looked upon by us as

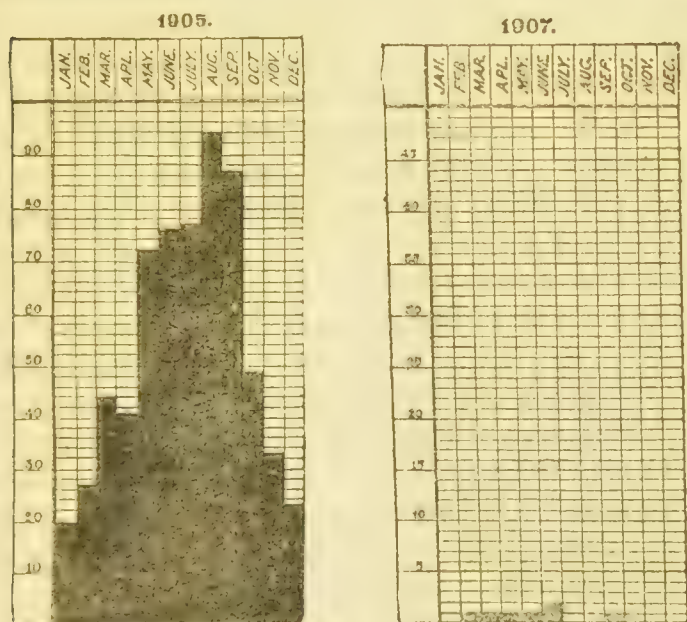
the most refractory and the most insusceptible animal we could imagine. We do not consider that the goat is readily susceptible to human diseases; it does not even take tuberculosis, which is such a common animal disease; so that we look upon the goat as the animal that is about furthest away from taking a human disease; and, as only the monkey took this disease, and not the rabbit, guinea-pig, rat, or dog, we considered that it was very improbable that a goat would take it. But, as a matter of routine, some goats were taken and inoculated under the skin with the *micrococcus melitensis*, and some were also fed on a small quantity of the culture—that is to say, a little of this *micrococcus melitensis* is taken out of a tube and put on the food, so that the animal eats it. These animals were examined afterwards as to their temperature and general appearance, but nothing happened. Now, there is a more delicate way of knowing whether an animal is affected by an infectious disease: and that is by an examination of the blood of the animal with the micro-organism that causes it.¹ The blood of an animal which has been attacked by a particular micro-organism has the power of drawing all these micro-organisms into clumps. It is a very peculiar thing, but it is true, that if you take the blood of an infected goat and mix it with an emulsion of the *micrococcus melitensis*, in a few seconds all these micrococci are drawn together into little clumps or lumps. So when, on examination of the blood of these goats, this clumping was noticed, it was seen that something had occurred. The goats did not get the fever in the way that we consider

¹ The reference is to the test called Widal's reaction. If a drop of blood from a case of typhoid fever be added to some living germs of typhoid from a pure culture in a test-tube, the germs run together in clumps. Similarly, blood from a case of Malta fever causes the lumping of *micrococcus melitensis*.

characteristic of fevers. They got the fever, but did not show any external manifestations of that fever. The micro-organism did multiply in their bodies, but it did not give rise to any ill-health; it did not give rise to any fever. By looking at the goat you could not say that that goat was ill. It gave as much milk as a perfectly healthy goat; it was as fat and smooth-looking as a healthy goat; so that it was only by this blood-examination that it was suspected that something was occurring. We suspected that the stuff we put in had not died off, as ordinary non-pathogenic organisms would, when brought into contact with a non-susceptible animal, but that it had gained some ground and was multiplying to some extent; so that in June 1905 a careful examination was made into this question again. More goats were bought; and before injecting them, or before feeding them, their blood was examined as a matter of routine, to see that the blood was all right. And, curiously enough, goats bought at this time gave this same Malta fever reaction. And then the idea struck us that the goats, the ordinary goats of Malta, might be suffering from this fever. And, on examining the blood of these ordinary Maltese goats, the *micrococcus melitensis* was found in it; and on going further, and examining the milk of these apparently healthy goats, a certain percentage of them were found to contain that *micrococcus melitensis* living in their milk. And that, of course, threw a great flood of light at once on the whole question.

“That was the important discovery, because it at once explained various things in the epidemiology of the disease: that seasonal prevalence had little or nothing to do with it; that, whether it was a large town or small village, nothing in different methods of sanitation seemed to matter; the curious fact that officers were much more struck by this

fever than the privates was also explained; and, after more work, the Commission came to the conclusion that the only way that a man takes Malta fever in Malta is by the drinking of goats' milk. Perhaps once in a thousand times he may take it in some other way, but you can sweep all that aside; the drinking of goats' milk is the main path of infection, and naturally, of course, it is easy to



Cases of Malta Fever among our soldiers in 1905 and 1907.

stop to a great extent our sailors and soldiers drinking goats' milk; so that, when the thing was once brought to the attention of the authorities, at once the disease was blotted out of the garrison of Malta. At first there was a certain amount of hesitation, and an attempt was made to sterilise the milk by boiling, and that kind of thing; but we soon pointed out that while boiling milk, if it was properly done, of course would sterilise the milk, yet when

you are dealing with the natives of these countries you never can trust that the thing is done properly. There is a small test, by which you can tell whether milk has been boiled or not : and by carrying that in your pocket, and by going into the hospitals and demonstrating to the head of the hospital that his milk was not boiled when he thought it was, after a month or two of this the conclusion was come to that no goats' milk whatever was to enter any military or naval hospital, or any ship or regiment. The general gave an order that not a drop of goats' milk was to be used, and all the troops were put on condensed milk. The preventive measures came into being on July 1, 1906."

[The admissions into hospital, for Malta fever, among the troops in Malta, had been 404 in 1903, 320 in 1904, and 643 in 1905. No less than 21 officers and 382 N.C.O.'s and men were invalided home from Malta to England, in 1905, for Malta fever. In 1906, up to July 1, there were 123 cases. During the rest of 1906, including the three worst months for the fever, there were 40 cases ; in 1907 there were 11 cases ; in 1908 there were 5 cases ; in 1909 there was 1 case. In 1910, up to the time of the statement made in the House of Commons (*The Times*, June 14, 1910) by the Under Secretary of State for the Colonies, there were no cases.]

Sir David Bruce was asked, Do the natives suffer from fever? He answered: "Yes; but, of course, not to the same extent, apparently, that the soldier suffers. The soldier goes there a susceptible man; the Maltese are always living among it. I imagine that nearly every Maltese has this fever at one time or another; but they drink it with their mothers' milk, and they have it when they are young, so that they become, as it were, immune to it. . . . But, of course, the diagnosis of the fever away in these country villages by country doctors, and the

desire of those people not to offend their patients, and all that kind of thing, make the notification returns very unreliable. . . . I have a letter from Dr. Zammit, who is a member of the Board of Health, and he informs me that the number of cases of Malta fever in the native population is as great this year as ever. The goat continues to be used for milk absolutely. The Government has been able to do very little yet. Malta is a very conservative place."

Nagana

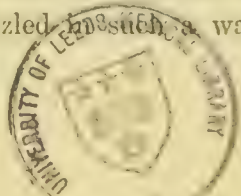
Sir David Bruce also gave evidence on nagana (tsetse-fly disease) and on sleeping sickness. "I went up to Zululand," he said, "in the end of 1894. The tsetse-fly disease, before that, was looked upon as being a disease caused by the poisonous bite of a particular fly, just as a snake bite would poison, or as a wasp is said sometimes to kill people. When I went up to Zululand, the natives sent me in a certain number of cattle suffering from nagana. They looked very ill, but one did not know anything about their illness. The only thing to do was to examine them systematically. One of the easiest tissues of the body to get at for examination, of course, is the blood; so that one makes a very careful examination, in all sorts of ways, of the blood of the animal: and, while I was doing that, I came across a peculiarly shaped thing in the blood; it was very scarce in the blood of these cattle, but looked as if it was something foreign to the blood. At that time, of course, one was working quite in the dark.

"In nagana in cattle, this particular micro-organism is very scanty: you may examine the blood for several days very carefully, without finding one; but in some other species of animals they grow in much greater numbers

in the blood. So that when I took, as a matter of routine, some of the blood of the cattle, and put that blood into a dog, it was found that the dog rapidly became ill, that its blood was swarming with this peculiar-looking thing, and therefore there was probably some connection proved between this peculiar-looking thing and the disease. It is an acute disease, with dogs; they die in ten days or a fortnight; whereas cattle will live on for a year, and even recover in some few cases.

"The discovery that nagana and the tsetse-fly disease were the same, was a matter of accident. Within a few miles of the hill on which I was living was a fly belt; and as I had read about this fly disease in books of travel, I was curious to know what the fly disease was. Accordingly, as a matter of curiosity, I sent down one or two horses, some cattle, and several dogs to live in this fly country for a fortnight; and when they were brought back to the top of the hill I examined them, and, much to my surprise, I found this same creature living in their blood; and then I began to suspect that nagana and tsetse-fly disease were probably the same, and that the disease was really caused by this trypanosome, this parasite. First of all, there is the small foreign-looking thing found in the blood of cattle, in very small numbers; you could not say that it had anything to do with the disease; but when you put a quantity of the blood into dogs and horses, you found that this body pressed very much to the front. Then you found out by accident that the animals coming from the tsetse-fly country have this trypanosome in their blood.

"The natives thought that the tsetse-fly disease was caused by eating the vegetation in these unhealthy places, especially where there was wild game. So in order to experiment further, I sent down horses into this fly country for a certain time, muzzled in such a way that they could not



eat or drink anything. They were taken down in the morning, brought back at night, and not allowed to drink water or eat a blade of grass. The horses which were sent down in this way came back to the top of the hill, and developed the same parasite in their blood. Invariably we found this trypanosome in their blood; and in three weeks or so they died of this disease. Then, naturally, one began to think that the tsetse fly had something to do with it, because the flies were biting them when they were down there. And one began experimenting by bringing up these flies in cages, feeding them on a dog suffering from nagana, and then, at various times afterwards, feeding them on healthy animals; after twelve, twenty-four, forty-eight, and sixty hours, and so on. Thus I discovered that this fly could carry this trypanosome from an infected animal to a healthy animal, readily, up to forty-eight hours; so that one could see, in the case of the animals going down to the fly country and being bitten, that if they were bitten by flies even forty-eight hours after the flies had fed on an infected animal, it might give rise to the disease. Then another question arose. Were the flies themselves, as you found them in the fly country, capable of giving rise to this disease, without any feeding on infected animals up above?

“Accordingly, I brought up cages full of these tsetse flies to the top of the hill, and put them straightway on healthy animals; and the healthy animals all got the same disease. Therefore, the fly down in the fly country was infected with this parasite, and could give it to any domestic animal that came near. Then one naturally wondered where these things got this little parasite from; and, in order to find that out, further experiments were made.

“The flies feed, of course, upon the wild animals; what more natural, then, than that they got this parasite from

the wild animals? The wild animals are perfectly healthy-looking and sleek, and do not appear to be suffering from anything. Accordingly I went down and shot many wild animals: buffaloes, koodoos, wildebeeste, and others; and the moment an animal was killed I sent up a bottle full of its blood to the top of the hill, where there was no nagana at the time, and injected that blood into healthy dogs. A certain number of these dogs took nagana, and I found this same parasite in their blood. In this way I showed that these wild animals had this thing in their blood; and afterwards, to make this more sure, by patiently looking for a long time microscopically at the blood of these wild animals, I discovered the thing itself in their blood—in small numbers, of course; in large numbers it would affect their health. In small numbers, so far as I know, it does not affect their health. So that there, again, by animal experimentation, it was proved that the wild animals act as a reservoir of this particular parasite; and I do not see how one could have proved it otherwise. Even when one knew that the parasite was in the blood, it took a whole day of examination of very many specimens to find a single parasite with the microscope; and one would not have examined so long and so carefully unless one knew that one would find it if one had patience."

Asked whether animal experimentation had helped at present to discover any remedy for nagana, Sir David Bruce answered: "It was in 1895 I sent home a dog suffering from nagana to London, which dog arrived here alive: that is to say, as one dog died another was inoculated, and at last a dog was delivered in London with this living trypanosome in its blood; and these trypanosomes have been spread from London, from that one dog, to scientific workers all over the world, and there has been an immense amount of work done in the last ten or twelve

years with regard to these trypanosomes. There have been, I should say, thousands of animal experiments with a view to find a method of serum treatment, and quite an extraordinary number of experiments have been made to find a method of medicinal treatment; and, as you know from the daily press, within the last few days there has been discovered a powerful drug, one injection of which, for example, into a guinea-pig which is just dying of this disease, which would not have ten minutes to live (I do not think I am exaggerating), which is at death's door, will drive all the trypanosomes out of its blood, and in a day or two that animal will appear healthy; and animals so treated have remained without trypanosomes in their blood for, I think, two months. . . .

“When I was in Zululand, in 1894 and 1895, I tried arsenic on horses affected with nagana, and I was able to keep these horses alive for a year by giving them arsenic in their food. As a rule, horses die in about three weeks of this acute disease. But, at the end of the year, the trypanosomes came back into the blood; and although one doubled or trebled the dose of arsenic in the animal's food, the trypanosomes paid no longer any attention to the arsenic, but multiplied, and killed the animal; and in that condition our knowledge has remained till within the last year or two. Lately a great deal of work has been done, especially in regard to sleeping sickness; and now the idea is that, by giving an arsenical preparation first, and killing all those parasites that are susceptible to arsenic, you can kill off the remainder by giving them another poison. The poison used has been first atoxyl and then a preparation of mercury; and no doubt Mr. Plummer has kept animals alive in this way for some 300 days. These rats ought to die in 5 days. The Liverpool School of Tropical Medicine have got much the same result. It

is a question whether we can say that these animals are really cured; but, however sceptical I may be, I imagine that when a rat lives 300 days instead of 5, it looks very much like a cure."

Sleeping Sickness

Sir David Bruce was then asked about sleeping sickness, and gave an account of the work up to 1903, at which time the discovery began to be made that sleeping sickness was due to the presence of a trypanosome in the blood and the cerebro-spinal fluid of man. "We examined some thirty cases of sleeping sickness for trypanosomes, and for trypanosomes alone: we gave the natives chloroform, and removed about an ounce of fluid from the vertebral canal, and had it centrifuged in order to throw any little solid bodies down to the bottom. I examined the sediment, and in most of these cases we found the trypanosome. In fact, after I went into the thing more carefully, this trypanosome was found in the cerebro-spinal fluid in every case of sleeping sickness examined. I put aside the idea of sleeping sickness being caused by a bacterium, and went for the trypanosome. After finding it in 100 per cent. in the cerebro-spinal fluid, we went on to examine the blood. Just as in the cattle in nagana, the trypanosomes are exceedingly scarce in the blood in sleeping sickness: and it is only by very patient work, and examining large quantities of blood, that you can find them. In this way we found this same trypanosome in every case in the blood, except on one occasion. So we had now learned the fact that in the blood of sleeping-sickness cases, and in the cerebro-spinal fluid, we could always find this parasite. The next point we investigated was this. If this parasite is an ordinary harmless guest of the people of this country, then it will be found in the natives who live outside the

sleeping-sickness area, as well as in those who live within this area. Just as in nagana you have a fly country, so in sleeping sickness you have a sleeping-sickness area, outside which the natives will not suffer from sleeping sickness. So we examined some two hundred cases of natives from a region outside the sleeping-sickness area, and we could not find this parasite in a single case. Then it began to look as if this trypanosome had really something to do with the disease.

“Accordingly we took cerebro-spinal fluid containing these living creatures, and injected this fluid into the ordinary animals we found round about. The monkey, of course, is very common there, and so are dogs and cattle. The blood of these monkeys was examined every day, and their temperature was taken very carefully, and they were watched. After five or six days, the same trypanosome appeared in their blood, and we found it daily for some time. Sleeping sickness is a very slow, chronic disease; and, at the end of three, four, and five months, these monkeys began to show the same kind of symptoms that men show: they became very lethargic, and sat all day as it were with their heads on their knees, and in the course of time these monkeys died. After death we examined the cerebro-spinal fluid, and we found this trypanosome in it, as we had found it in the blood. In man there is a particular change in the brain, first described by Dr. Mott; so that, if you cut a section of the brain, you can tell by the appearance of it that he has died from sleeping sickness. If these monkeys died of the same disease, you would naturally expect to find this peculiar anatomical change of the brain. This anatomical change of the brain was found: first, I believe, by Captain Harvey, R.A.M.C. And therefore the conclusion has been come to that the disease of which the monkeys died is the same as the

disease in man, and that in all probability the trypanosome is the cause.

“Then the next point to investigate was, how are these natives infected: and, from my old work on nagana, I at once thought that a tsetse fly would be the carrier of the trypanosome. At that time it was not known that there were any tsetse flies in Uganda: but when we went down to the lake shore and began to hunt for them, we found a particular species of tsetse fly, different from the nagana one, present and in large numbers. We caught these flies, kept them in cages, fed them on sleeping-sickness natives, and then on healthy monkeys, and we found that these tsetse flies could carry sleeping sickness from a sick native to a healthy monkey.

“And then we made the further experiment. As there were a great number of infected natives on the lake shore, we collected these flies from among these natives, brought them up to the laboratory, and put them straight-way on healthy monkeys: and we found that these monkeys took the disease. Then the next thing we did was to investigate the distribution of the disease. The distribution of sleeping sickness in Uganda is very extraordinary. It is confined to a narrow belt running along the margin of the Lake Victoria and its islands. Further, the disease is only found where there are forests. You do not find sleeping sickness where grass plains come down to the shore. You find no sleeping sickness if there is a belt of papyrus reeds growing along the lake shore. In the interior of the country there is no sleeping sickness. It is a very peculiar distribution. Therefore, I said to myself, If this tsetse fly carries it, very probably this fly has also a peculiar distribution. (The tsetse fly which carries nagana is spread all over the country.)

“Then we got hold of the native chiefs, and they set

their minor chiefs all over Uganda to collect flies, and within a couple of months they sent in about a thousand collections of flies from all over the country. At the same time, we asked them to send in a word as to whether sleeping sickness was found where the flies were found. Then we separated all these biting flies into two categories, namely, this particular tsetse fly and the others; and we took two large maps, and whenever a collection of flies came in from a place containing one or more of these tsetse flies, we put a red mark on that spot on the map. We also had another map of the same size, and if a chief reported that sleeping sickness was found at a particular place, we put a red mark on the same place on the second map. This went on for some months, until at last we had about a thousand observations: and then, when you looked at the two maps, you found that the red spots on one map coincided with the red spots on the other map; and the blue discs, which we put on the places where no tsetse flies were found, coincided exactly with the places where no sleeping sickness was found. Accordingly we came to the conclusion, that the distributions of sleeping sickness and of this particular species of tsetse fly were identical; and that gave us another proof that this trypanosome was the cause of the disease, and that there was only one way by which man could become infected; and that, by being exposed to the bites of this particular species of tsetse fly. Since then a good deal of work has been done: but that is now, I think, established beyond doubt."

In answer to further questions, Sir David Bruce stated that this species of tsetse fly conveys the trypanosome of sleeping sickness from man to man. The tsetse fly of nagana conveys the trypanosome of nagana from animals to man, but this trypanosome of nagana dies off in man: it does not live in his blood. It was easy to see living

trypanosomes in the stomach and proboscis of a tsetse fly recently fed on an infected animal. The distribution of the flies evidently depended on the presence of clear water. "If you have a river full of reeds, a marshy river, which is a common kind of river in Uganda, the fly never goes up there: it requires a river which has always lots of water in it. It must have forest and clear water; it will not live in grass alongside water; it will not live in a banana plantation alongside water. It lives where there are tall trees and jungle and clear water." Asked how far away it flies, he said that in Uganda he did not think they were found, as a rule, more than a hundred and fifty yards away. If you cleared away the jungle close to the water, you would clear away the fly. That was being done at Entebbe, and at ferries and such-like places.

Finally, Sir David Bruce referred to the possibility of removing natives from the areas of the disease. "I think myself that in a place like Uganda, especially after the population has been so reduced, if the Government and the chiefs made a big effort, they could take the whole of the natives out of the sleeping-sickness areas and put them in healthy places; and if they could do that, theoretically in forty-eight hours there would be no more sleeping sickness, there would be no fresh cases of sleeping sickness in Uganda. The tsetse fly only carries infection for forty-eight hours: and if you could remove the reservoir of the disease, in forty-eight hours every *Glossina palpalis* in Uganda would be harmless; and if you did not expose affected people to the tsetse fly again, then sleeping sickness would be blotted out by a wave of the magician's wand sort of business. It is mainly a question of money. If you know the distribution of the fly and you know the natural history of the disease, you can do a great deal. The great thing in all these cases is to know the natural history from beginning

to end, and not till then to try to do anything. You require to keep the people away from the sleeping-sickness areas until the healthy-looking ones have developed the disease. You require to keep them away in the interior of the country. Those who are affected by sleeping sickness must of course never go back to the fly area. If at the end of forty-eight hours you sent back the population to the lake shore, you would not be able to distinguish between the healthy and the unhealthy; you require to keep the place clear for some time. But I think at the present time it is not out of practical politics to keep the fly area of Uganda clear of population for a certain time. The Uganda natives are docile, and their chiefs are intelligent people, so that I would not look upon it now as outside practical politics to stamp out sleeping-sickness in Uganda."

NOTE.—This evidence was given in Nov. 1907. Two and a half years later, June 3, 1910, at the Annual General Meeting of the Research Defence Society, Sir David Bruce was able to report that his hope had been fulfilled. "In 1903, when we called a meeting of the Uganda chiefs together, to explain to them that sleeping sickness was caused by the entrance of a blood-parasite into their bodies, that this was carried from the sick to the healthy by the *kivu* or tsetse fly, and that the way to stop the sleeping sickness was to remove the natives from the fly-area, they politely disbelieved us, saying that the fly had always been on the lake shore in their time and in their fathers' time, and that therefore it was harmless. I then said to Sir Apolo Kagwa, the Prime Minister, that if he didn't believe the fly carried sleeping sickness, he should go and sit among them, under the shade of the trees on the lake shore, in order to show every one the faith he

had in the innocence of the fly; but he answered, *God forbid*. But these same so-called uncivilised natives, whose untutored minds couldn't perhaps at once grasp the position, a few years later were so convinced of the truth of what was told them, that they cleared the lake shore and islands of their inhabitants, with the result that, so far as I am aware, at present not a single new case of sleeping sickness is being contracted in Uganda proper, and the toll of human lives to this plague has ceased to be paid. This toll has been estimated at 200,000 out of a population of 300,000. In one island alone, Buvuma, with a population of 22,000, 18,000 are reported to have perished. What a saving of human life by the simple means of removing the population from the fly-area: and this knowledge acquired mainly by animal experimentation!"

SIR W. OSLER, Bt., M.D., F.R.S., *Nov. 20, 1907*

Sir W. Osler, Regius Professor of Medicine in Oxford, and sometime Professor at the Johns Hopkins University, Baltimore, said that he fully agreed with the general line of the medical and physiological evidence, but he would like to add one or two points, which perhaps had not been fully dealt with by other witnesses.

Yellow Fever and Malaria

"I think that the story of yellow fever illustrates, perhaps more satisfactorily than any other, the remarkable way in which experiments, carefully devised and carried out, may influence not only our knowledge of the etiology of a disease, but may influence extensively the commercial relations of nations, and save not only thousands of lives,

MALARIA AND YELLOW FEVER



The mosquito-proof hut near Ostia, in which Sambon and Low and Terzi lived through the malaria season, 1900, without taking a grain of quinine, and without being infected with malaria. See the paper by Sir Patrick Manson, K.C.M.G., M.D., F.R.S., in the *British Medical Journal*, September 1900.



The mosquito hut at Camp Lazear, in which the American Commission in 1900 proved that yellow fever is conveyed, not by infected clothing, but by infected mosquitoes. See the "Life of Walter Reed," by H. A. Kelly, M.D.

but millions of pounds annually. Yellow fever has been the great scourge of the regions round the Caribbean Sea, more particularly Mexico, the West Indian Islands, Brazil; and every few years it has spread into the Southern States of America, and occasionally has reached Philadelphia, and even as far north as Boston. In the early part of the last century, on several occasions, it reached Europe, and there were extensive outbreaks in Spain, costing some thousands of lives.

“Many attempts had been made to find out the cause of the disease; but all had failed up to the year 1900, when a Commission was sent to Havannah by the United States Government, especially to investigate the cause of yellow fever. That Commission, composed of Drs. Walter Reed, Carroll, Lazear, and Agramonte, recognised particularly the relations of the mosquito to the disease; and they went out with the specific object of determining, if possible, to discover the germs of the disease. The experiments which they devised were carried out in a United States Army Camp in Havannah; and they are among the most remarkable that have ever been made. The camp was entirely isolated, so that there could be no possibility of communication with the outside. It was composed of a certain number of immunes—that is to say, persons who were no longer susceptible to yellow fever, in consequence of having had it—and of non-immunes. That is a common division of the population in Brazil and Havannah, namely, into immunes and non-immunes. A man is asked whether he is immune or not.

“In this camp, a house was constructed with two compartments, divided from each other by a wire mosquito-proof screen. There were two sets of experiments made in connection with this little house. In the first place, into one side of this hut fifteen infected mosquitoes were placed.

Those were mosquitoes that had bitten a yellow-fever patient within the first three days of the illness. Men were selected, partly from the Army and partly from civil life, who had expressed and signed their willingness to submit themselves to experiments. I may state that one or two of the medical men also volunteered. Into the compartment with the fifteen mosquitoes a non-immune went, in the morning, in the afternoon, and on the following morning, and submitted himself to their bite; I do not know how long, but long enough to get a number of bites. Within five days he had the disease. At the same time, in the adjacent compartment, which was simply screened from these mosquitoes by a wire netting, for twenty-one consecutive nights two non-immunes slept. They did not get the disease. Experiments of that nature were repeated on several occasions, demonstrating quite conclusively that so long as these infected mosquitoes were kept from biting, though there was only a screen between them, the individuals did not get yellow fever.

“Then, experiments were made, on a very extensive scale, to determine whether the disease was conveyed by means of fomites: that is to say, whether, as was usually supposed, the disease was carried by infected clothing, and by the excreta of the patients, and by the vomit. For that purpose, the clothing and material, soiled by the vomit, and by the blood, and by the stools of the patients, were placed in one of these rooms; and a group of non-immunes slept in contact with this clothing—in some instances between the actual sheets of the beds in which these patients had died—for twenty-one consecutive nights. That experiment was repeated with a second set of non-immunes, sleeping, as I say, with the bed-linen and with the soiled materials of patients who had died of the disease. Not one of them took yellow fever. Then these men were

subsequently experimented upon by placing them in the section of the house with the infected mosquitoes, and in each instance they took the disease.

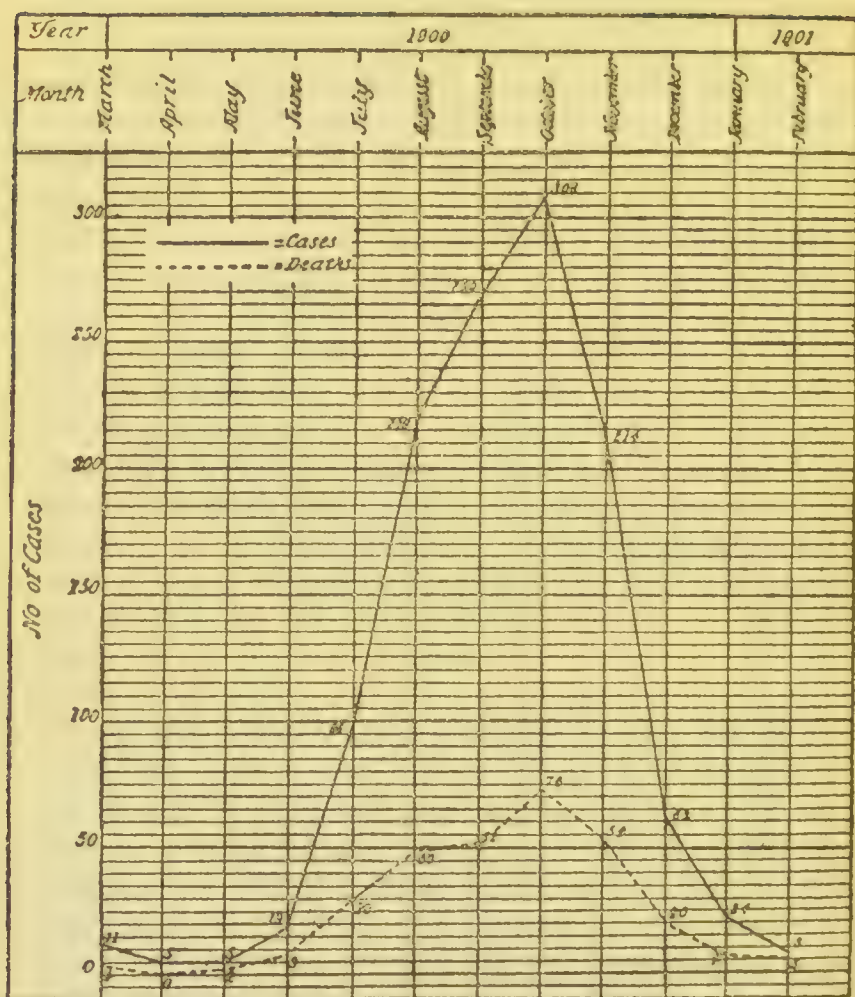
“Altogether twenty-two soldiers submitted themselves to the experiments, and twenty-two took the disease: fortunately none of those cases proved fatal. One fatal case was a former assistant, Dr. Lazear, who had been for several years in charge of my clinical laboratory. He submitted himself to the bite of an infected mosquito, and three days subsequently developed the disease and died.

“The mosquito, to become infective, must bite the yellow-fever patient within the first three days of the patient having the disease. The mosquito itself is not infective under a period of twelve days: the mosquito may bite an individual anywhere up to the twelfth day after receiving the infection, without being infective. Then it remains infective all through the rest of its life.

“Of course, the interesting practical point comes out, that this series of experiments has already revolutionised life in those regions. Havannah within the next two years was cleared of yellow fever, the first time in the 300 years of its existence. . . . This is the kind of discovery that will revolutionise conditions of life in the Tropics. The discovery of the malarial parasite, and the discovery of the relations of yellow fever with the mosquito, will enable the Panama Canal to be built. Without those two investigations, the probability is that it could not be built: it would cost an enormous sacrifice of human life, just as happened with the French. Now, there are 20,000 whites on the Isthmus at work; of course, nearly all of these are non-immune. There has been practically no yellow fever, and, what is much more important—because it was not the yellow fever that killed the French to the same extent—there is no malaria.

YELLOW FEVER

I

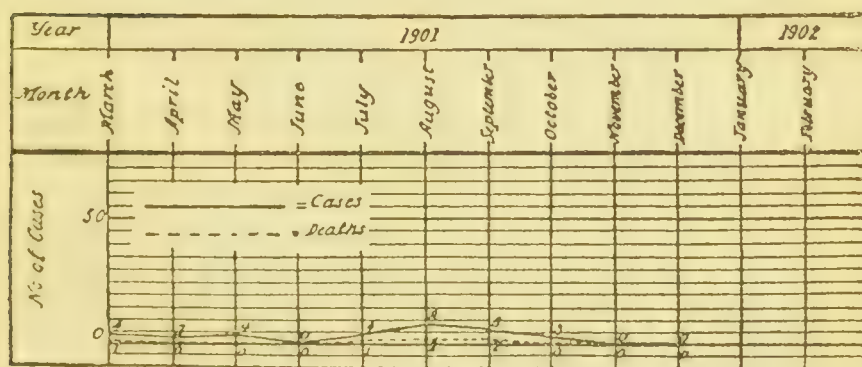


This chart shows the progress of yellow fever in Havana during the epidemic year ending March 1, 1901, when the sanitary authorities were putting forth every effort known to sanitary science to control the disease. This was in the "pre-mosquito period." The continuous line gives the number of cases; the dotted line gives the mortality. (From Dr. Kelly's "Life of Walter Reed," New York: McClure, Phillips & Co., 1907.)

"I am referring to these experiments only as an illustration that it is through the experimental side of medicine, the experimental spirit in medicine, that these great revolutions have been effected, revolutions with which there is nothing else in human endeavour to compare, from the standpoint of humanity. There is not anything else in the whole development of the British nation that is

YELLOW FEVER

II



This chart shows the occurrence of yellow fever in Havana for the epidemic year, March 1, 1901, to March 1, 1902, during which year the disease was fought on the mosquito theory. The continuous line gives the number of cases; the dotted line gives the mortality. (From Dr. Kelly's "Life of Walter Reed.")

going to have so much importance as the discovery of the mode of the transmission of malaria. It is going to make the Tropics habitable. And all this has come about through the experimental method and the experimental spirit. Without these, such investigations could not have been made, and these perfectly phenomenal results could not have been achieved. It was the same spirit that gave us anaesthesia, and the same spirit that has given us antiseptic

surgery, and the same spirit that has given us preventive medicine: three things which stand out in the record of human achievement with which nothing else may be compared—I mean from the standpoint of everyday, common humanity. This experimental investigation into the interaction between the mosquito and man producing yellow fever would never have been thought of, if it had not been for previous experiments on animals. The men who made these investigations spent their lives in laboratories, and their whole work has been based on experimentation on animals. They could not otherwise, of course, have ventured to devise a series of experiments of this sort.”

NOTE.—As one species of mosquito conveys yellow fever from man to man, so another species of mosquito conveys malaria from man to man. In the long series of observations on malaria, three facts are especially to be noted: (1) In 1898, in India, by experiments with mosquitoes and small birds, Ross discovered the germs of “bird-malaria” in the stomach of the mosquito, and was able to infect healthy birds by causing the infected mosquito to bite them; (2) In 1900, Sambon and Low and Terzi set up a mosquito-proof hut near Ostia, in a part of the Campagna that was saturated with malaria. They lived in this hut through the whole of the malaria season; they took not a grain of quinine, and they had no malaria. A similar experiment, on a large scale, was made by Grassi among the workmen and their families on the Battipaglia-Reggio railway. (3) In 1900, also, a consignment of mosquitoes, fed on a case of malaria in Rome, was sent to the London School of Tropical Medicine, and Dr. Manson, Sir Patrick Manson’s son, and Dr. Warren submitted themselves to be bitten. They both developed the disease, and the germs were found in their blood.

The campaign against malaria and yellow fever, therefore, is a campaign against the mosquito, *plus* such measures as the segregation of the white man's houses from the native huts, the State-aided distribution of quinine, and the general use of mosquito netting, etc. The work of the "mosquito brigades" is to destroy or remove the haunts and breeding-places of the mosquito. It breeds in little collections of stagnant water, laying its eggs on the water: ponds, swamps, puddles, roadside ditches, tanks, cisterns, and all such chance receptacles of rain-water as rain-barrels, pots and pans and broken bottles and old biscuit-tins—all the rubbish of the backyard. Pools and ditches are drained, or stocked with minnows, or filmed with kerosene to kill the larvæ; broken crockery and the like débris are carted away; cisterns and wells and rain-barrels are properly protected; everywhere the surface soil is tidied up, and all collections of stagnant water are removed, or are set running, or are covered over.

The President of the United States, in his address to the Philadelphia Medical Club, May 4, 1911, gave a full account of the results achieved against yellow fever in Havana, tropical anæmia in Porto Rico, cholera, small-pox, plague, malaria, leprosy, and beri-beri in the Philippines, yellow fever and malaria in the Panama Zone, and typhoid fever in Texas and California. This address will be issued in pamphlet form by the Research Defence Society.

VIII

DISEASES OF ANIMALS. VETERINARY SURGERY

MR. STEWART STOCKMAN, *December 5 and 12, 1906*

MR. STEWART STOCKMAN, M.R.C.V.S., Chief Veterinary Officer of the Board of Agriculture and Fisheries, formerly Principal Veterinary Surgeon in the Transvaal Agricultural Department, said that there were two kinds of contagious diseases in animals which caused serious ill-health and loss of life: these were the bacterial diseases, and the parasitic diseases. He pointed out that the parasitic diseases, such as tape-worms, had come to be understood by experiments on animals; and said that without that understanding, measures for prevention would have been impossible. In the same way the measures for preventing trichinosis in man were arrived at by experiments on animals. He went on to speak of the diagnosis of certain contagious diseases of animals, such as anthrax, glanders, tuberculosis, swine-fever, and swine-erysipelas; sometimes this diagnosis was possible only by the help of inoculation experiments. He was then asked to state his experience of the action of preventive inoculation upon animal plagues. He answered, "The methods of inoculation have in every case been arrived at

and proved by experiments on animals, and most of the substances employed to produce immunity can only be prepared by inoculation experiments on animals."

With regard to rinderpest, he said that it had cost Great Britain, between 1865 and 1869, £1,119,994. "In 1897, the appearance of rinderpest in South Africa resulted in the disease being studied with a view to discovering a method of preventive inoculation. The investigations were successful, and the benefits obtained from anti-rinderpest serum are recognised in every country where the disease has appeared since the method was introduced. . . . After peace was declared in South Africa, rinderpest was one of the diseases against which the newly created Veterinary Department had to direct operations; and it was dealt with by the serum method. During the period in which I was Principal Veterinary Surgeon to the Transvaal Government, about 14 outbreaks were reported and stamped out in various parts of the Colony. . . . The final outbreak I am able to give more particulars about, because it occurred at the time the country was very much more settled; it occurred in a large native location, where the sick and in-contact animals numbered about 800. By the use of the serum the disease was stamped out, in the last affected herd, in about a month. The death-rate was 10 per cent., whereas it may be from 50 per cent. to 100 per cent. from the disease alone; and, of course, there may be a very much larger death-rate, on account of animals which have to be slaughtered to prevent the further spread of the disease, when the rinderpest has to be dealt with in the absence of the inoculation method. . . . The discovery of the serum method has been an enormous boon to South Africa."

With regard to contagious pleuro-pneumonia, he pointed out that, from September 1890 to the end of 1898, when

the disease was finally stamped out in Great Britain, 1,605 diseased cattle and 21,092 in-contact cattle were slaughtered, the cost in compensation being £357,626. "That was dealing with the disease by the stamping-out method, by slaughter. I would like to compare these results with what took place in the Transvaal after the war, and during the restocking operations, both of which resulted in pleuro-pneumonia being disseminated. The country could not afford to stamp the disease out by wholesale slaughter; but, in addition to the financial difficulty, one would not have been justified in advising it for a country where most of the farm work and local transport has to be carried on by oxen, and where the animal herds had already been reduced to an almost impossible number, owing to a long war coming on the top of the rinderpest. The Veterinary Department advised that pleuro-pneumonia be dealt with by slaughter of the affected, and compulsory inoculation of contact animals with a pure virus. . . . From May 1903 to June 1904, 256 outbreaks of the disease were dealt with in the above way; 741 affected animals died or were slaughtered, and 9,000 in-contact animals were inoculated. By the method of slaughtering contacts, the compensation payable would have been about £135,000. In the following year (1904-5) the results of inoculation became apparent, as only 13 outbreaks occurred, and the number of in-contacts which had to be inoculated fell to 3,109."

With regard to tetanus, he said: "There are regions, all over the world, where the soil is almost grossly infected with the spores of the tetanus bacillus; and there are farms which are particularly infected with this microbe, which lives in the soil. If the animals in these districts are the subjects of accidental wounds, or those produced by surgical operations, a large number of them

die of tetanus. In the ordinary course of stock-breeding, it is necessary to operate annually on a large number of farm animals; for example, most young males have to be castrated. Some regions are so badly infected that it is almost a certainty that an animal, with a wound in a part of its body which is likely to come into contact with the soil, will take tetanus, and probably die. In 1897 Professor Nocard demonstrated, by experiments on about 30 horses, that tetanus could not be produced in them by inoculation of virus, provided they had received a dose of antitoxic serum not later than three or four days after they had been inoculated with the virus. Basing his ideas for the prevention of tetanus on these experimental observations, he furnished several veterinary surgeons, practising in infected districts, with quantities of serum, doses of which were to be injected into animals before the performance of surgical operations, and into those which had received accidental wounds. Records of the results were furnished in connection with 2,705 animals. In 2,300 cases serum was administered immediately after an operation; and no death from tetanus occurred in these animals. Of the remainder, 400 received serum from one to four days or more after an accidental wound on dangerous parts (that is, parts of the body near the soil). Only one case of tetanus occurred in this lot of animals, and it ended by recovery. These observations were purposely made in tetanus-infected districts at the request of veterinary surgeons, who annually lost numerous patients from tetanus; and, during the period of observation, 259 cases of tetanus were observed in untreated animals, so that there could be no doubt that the tetanus spore was present in an active condition at the time of the observations. As a result of these observations, the prevention of tetanus by serum has been successfully

adopted all over the world; and there are many observations from individual practitioners, which testify further to the efficacy of this method of prevention." Asked what part of England he would call a "tetanus-infected district," he said that there were several farms in England which were pretty bad; but he had more in his mind parts of India. Round about Bombay, and round about Calcutta, it was exceedingly dangerous to do an operation on an animal, especially about the lower parts of the body.

With regard to anthrax, he said that a method of preventive inoculation, which was effective for all commercial purposes, was devised by Pasteur. Within the last two or three years, the original method had been somewhat modified. He did not think the modifications were of any great importance. He quoted Chamberland's paper, in the *Annals of the Pasteur Institute*, 1887, showing that the death-rate had been reduced from 10 per cent. of the whole stock, on farms attacked, to 0.91 per cent. "Statistics collected in Hungary, on over 11½ millions of inoculated animals, show that the results have been practically the same as in France. They talk of farms where the loss, which was 10 per cent., has been reduced to under 1 per cent."

With regard to blackquarter, he said that the method of Arloing, Cornevin, and Thomas, and its modifications, had given most excellent results in various parts of the world. "The statistics available deal with hundreds of thousands of animals. A very large number have been treated by this method, and may therefore be accepted as satisfactory evidence of its efficacy. They show that, by this protective method, the death-rate from blackquarter in badly infected districts has been reduced, in the inoculated, as much as 14 per cent. in some cases; and,

although the actual reduction of the death-rate varies in different districts, it is always less in those animals which have been inoculated—that is to say, in infected districts. There seems, moreover, great probability of the death-rate being further reduced by improvements in the method.”

With regard to swine erysipelas, he said that a serum treatment had been very successfully used in practice. “Not only has this serum decidedly curative effects on sick animals treated with it in the early stages, but a pig-owner can render his whole herd of animals immune by giving them a mild attack of the disease, which can be accomplished by injecting them simultaneously with the culture of the causal microbe, and a dose of protective serum. . . . The statistics from Hungary show that on 4,000,000 observations the death-rate was reduced in inoculated animals to 1.6 per cent.; whereas in the non-inoculated it amounted to about 20 per cent. . . . In Eastern Prussia, there were issued, in 1898, records of observations on 22,161 pigs, 3,831 of which were made on farms already infected at the time of the inoculation; the disease had already broken out on these farms. In all of the latter, the disease ceased to spread after protective inoculation, while 58 per cent. of the actually sick pigs which were treated with serum recovered. In Würtemberg, nearly 18,000 pigs in infected districts were inoculated; of these only 6 afterwards died of the disease, whereas 3,254 of their companions, which were not inoculated, died of swine erysipelas.”

Mr. Stockman went on to speak of the tropical diseases of animals. “Already it has been found possible to give mules a high degree of immunity against South African horse-sickness, a seasonal disease which, in some parts of Africa, annihilates practically every horse in a district,

and makes the settlement of these districts almost impossible. Advances have also been made in the prevention of such diseases as red-water, heart-water, and blue-tongue. . . . It is absolutely necessary, for the grading up and development of stock in these more or less new countries like South Africa, that pedigree-animals should be imported from the most highly developed herds or flocks in Europe. When these high-priced animals are imported for this purpose and placed on the pastures, it has unfortunately been found that from 50 to 90 per cent. die of the above-mentioned diseases before much good can be derived from their introduction. The expense on this account becomes so enormous as to make it financially impossible for most farmers to import. . . . I have been informed by a responsible colonist from Southern Nigeria that the indigenous diseases of the country have practically annihilated domestic animals, which are necessary for the building up of a community. It is represented that transport work by draught animals is practically impossible. It is said that owing to the absence of milch-animals the native women have to suckle their children for a period of two years, and that the growth of population is thereby greatly interfered with. It is further stated that the infant mortality in the country is great, and that this is largely attributed to the absence of milk. I do not think that any remedy for this reported state of affairs can be found, except by experimental researches conducted with a view of getting some method of preventing mortality amongst the animals of the farm, the domesticated animals." Such experiments were now going on in the Board of Agriculture Laboratories.

With regard to tubercle and glanders, Mr. Stockman pointed out that these two diseases were communicable to human beings. "One of the most dangerous character-

istics of these two diseases is that they may exist in animals in an occult form, which ordinary methods of diagnosis completely fail to discover." Happily, experiments on animals had led to the use of tuberculin and of mallein as positive tests. "A horse affected with glanders will react to mallein, and an animal affected with tuberculosis will react to tuberculin. The practical utility of these substances has been, and could only have been, demonstrated by experiments on animals. Medically speaking, their discovery makes it possible to completely stamp out tuberculosis and glanders; that is to say, you can get an accurate diagnosis."

The following questions were put by Sir William Church and Dr. Gaskell:

Sir William Church: In reply to the Chairman, you stated that almost the only cutting operations that you do are in connection with drawing a little blood?—*A*. Yes.

Q. I think he thought it was blood that would be used for further experiments; but would you mind telling him the quantity that you sometimes have to draw, for serum, for instance, from a horse?—*A*. One or two litres.

Q. Or even more, sometimes?—*A*. I never draw more; but one might have to draw more.

Q. What is the condition of a horse after that blood has been withdrawn?—*A*. I have never seen any big animal like a horse, after withdrawal of two litres of blood from it, suffer any inconvenience.

Q. It takes its food the same day?—*A*. Yes.

Q. And does not appear to be in any way incommoded?—*A*. It does not appear to be incommoded by it at all.

Dr. Gaskell: Is not the horse often feeding while the blood is being drawn off?—*A*. Yes; it does not seem to pay any attention to it.

Mr. Stockman was then cross-examined by Sir William Collins. He said that he was not familiar with the Report of the 1888 Committee appointed to inquire into pleuropneumonia and tuberculosis. Asked how he could discriminate between reduction of cattle plague and pleuropneumonia by slaughter, and reduction by inoculation, he said that in cattle plague it was easy to discriminate. "If you take what has happened in Africa, and to a less extent in India, many men there, as here, have an absolute prejudice against slaughter; they will not do it; they have never slaughtered any of their cattle at all to prevent disease spreading. They started with compulsory slaughter in Africa, and then gave it up as hopeless.

Q. In those farms or districts, was it possible completely to control cattle plague without slaughter?—*A.* All I can say is, that the death-rate was, as I have said, much smaller, and then the disease disappeared under the inoculation régime.

Q. But your object is to stamp it out, I understand?—*A.* Yes, and it was stamped out.

Q. Without recourse to slaughter?—*A.* Without recourse to slaughter."

Asked about sheep-pox, Mr. Stockman said that the organism described in 1874 was not the cause of the disease. Asked about anthrax, and Professor Müller's opinion against Pasteur's vaccine, he answered: "I disagree with him in that. But I should like to explain that it may not always be worth one's while to inoculate; that is to say, on a farm where a few cases occur every year, you would not inoculate, and the best method in such a case would be, as he says, to destroy the carcasses and all sources of the infection, and prevent infection being brought in. But then there are countries where you cannot do that, and there are places which are already

so badly infected that, even if you do destroy the carcasses, the disease will go on, since the soil is badly contaminated." Asked about swine-erysipelas, he said that slaughter and isolation had some effect. But he did not think they had any very great effect; because the cause was a microbe of the soil. "Given an outbreak of the disease on one farm, you must not allow the animals to go on to another farm; but that would not stop the spread of swine-erysipelas." There had been accidents from inoculation against this disease; but his evidence was based on the average, taking these accidents into account.

Re-examined by Sir John McFadyean, Mr. Stockman stated that Nocard's work on pleuro-pneumonia was universally accepted throughout Europe: and, in consequence of Nocard's discoveries, very important improvements had been introduced in the method of inoculation. With regard to glanders, all our accurate knowledge of the cause of glanders had involved experiments on animals, nor would mallein ever have been discovered without them; and mallein was of immense practical value in dealing with outbreaks of glanders. He could see no way to prevent or minimise the danger of the spread of glanders but to use mallein. Again, with regard to anthrax, experiments on animals were necessary to detect anthrax in such materials as hides and imported foodstuffs. With regard to swine-erysipelas, the improved method of dealing with the disease, which was approved by the German Government and extensively practised in Germany instead of the Pasteur method, was due to experiments on animals. With regard to tetanus, he was of opinion that a horse could be absolutely immunised for a certain time against the disease, and that cases of tetanus could practically be abolished from veterinary practice. With regard to swine-fever, it was by experiments on animals that the

fallacies of previous bacteriologists had been disproved: and these experiments had been of great use to him for the accurate diagnosis of alleged cases of swine-fever in this country. At the present time, his own principal investigations were on epizootic abortion in animals, swine-fever, red-water, and anthrax.

Asked about the preparation of antitoxins, other than diphtheria antitoxin, he said that the animals did not seem much disturbed by it: they seemed to eat all right: he could not say that he had seen any material pain. If they had pain, they did not demonstrate it in any visible way. Asked about rabies in the rabbit, he said that the inoculation was done under an anæsthetic, and that the disease usually was of the paralytic form, without acute brain disturbance. Asked about tetanus, he said that there were places, like Calcutta, where tetanus was so common that bicyclists were warned to wear gloves along the roads. The protective use of tetanus-antitoxin for horses gave them no inconvenience, produced no fever, and never led to a fatal result. Asked about glanders, he said that the use of mallein was the ordinary method for diagnosis.

Asked about isolation and slaughter, he said that in this country, being an island, isolation and slaughter were applicable, because importations could be controlled: but, in other countries, isolation and slaughter would mean a fearful waste of money in some cases. Asked what he would advise if rinderpest broke out in England now, he answered, "If we got hold of the first outbreak, or the first one or two outbreaks, I should advise them to slaughter everything—everything affected, and everything in contact. But if, as one finds in many cases, we did not get hold of the first outbreak, and there were half a dozen outbreaks going on at the same time in various

parts of the country, I should conclude that the disease had established itself, and I would advise not only to slaughter the affected lot, but, if possible, to make an immune ring, a ring of immunised animals, for a certain distance outside the actually infected area. But, even then, you have to prepare your serum. Nobody has a large stock of this anti-rinderpest serum; and it would take you some time before you could have it ready. By giving serum, it would save you slaughtering probably thousands and thousands of animals not exactly in immediate contact, but within a dangerous zone." Asked whether the use of any serum could be enforced, he said that there was no power of enforcing any of these things at present. In a new order which the Board of Agriculture was hoping to issue with regard to glanders, they recognised mallein as a diagnostic agent: and he thought it would be advisable that the Board of Agriculture should have power to order the inoculation of animals in certain events.

Cross-examined by Dr. Wilson, Mr. Stockman agreed that Professor Koch's very low estimate of the risk of milk from tuberculous cows had caused a great surprise. Asked about finding the germs of anthrax in animals dead of that disease, he said that sometimes it was very difficult, if the veterinary surgeon did not see the body till twenty-four hours after death. He did not admit that there were at present any decided or great improvements on Pasteur's preventive treatment against anthrax. Asked about the tuberculin test for cattle, he said that it was more used than ever to test cattle before exportation: and that it was even fraudulently used, the cow being inoculated with tuberculin three or four times before going to market. That was the reason why the certificate of testing had of late not been accepted in the Argentine. Asked about

the mallein test for glanders, he said that some countries made it a condition of exportation of all valuable horses; but the hundreds of worn-out old horses annually exported to the Continent were tested on the other side, in Belgium or Holland. Asked about the horses kept for diphtheria-antitoxin, he explained that they did not have clinical diphtheria: they did not have the diphtheritic throat: but they might have the effect of the toxin on the peripheral nervous system. Asked about the curative use of tetanus-antitoxin for horses actually attacked by tetanus, he said it was a question of cost: that was the whole question. Asked further about anthrax, he said that Selavo's serum, for the treatment of anthrax in man, had been spoken of very highly in a Home Office Report on industrial anthrax. Asked about Koch's preventive inoculation against African Coast fever among cattle, he said that he and Theiler, contrary to Dr. Koch, believed that this fever was not an inoculable disease; and that Koch's experiments had been, to his thinking, entirely on wrong lines.

Asked what would be the proportion of errors in the use of the mallein-test in this country, he said that he did not think it would be 10 per cent., even with a single test-inoculation. With two test-inoculations, it would be less. That was among animals in which glanders absolutely could not have been detected in any other way. Asked about the horse-sickness, he said that he and Theiler believed that it was now possible to protect mules against horse-sickness with a loss of something like 3 per cent. on an average. Asked again about the African Coast fever, he said that the fact of its transference by ticks had been discovered solely by experiments on animals. He added that he had produced, in England, two typical cases of African Coast fever through the agency of ticks sent from South Africa.

PROFESSOR HAMILTON, *December 17, 1907*

Dr. D. J. Hamilton, Professor of Pathology in the University of Aberdeen, gave special evidence about diseases of sheep. He had been Chairman of a Departmental Committee appointed in 1901 by the Board of Agriculture, to investigate louping-ill and braxy. "These two diseases of sheep are extremely disastrous, and the cause of enormous mortality all over Great Britain more or less, especially in Scotland, the northern counties of England, and also in Ireland." The mortality from braxy had been as high as 80 or even 100 per cent. over certain districts in certain farms: the mortality from louping-ill was usually not so high, but sometimes came up to 20 or 25 per cent. The witness described the symptoms of louping-ill, and his Committee's investigation of the cause of the disease. For a long time they were puzzled: the organs appeared to be practically healthy. He had examined thousands of samples of blood. He had learned nothing as to the cause of the disease from post-mortem examinations. The Commission had then proceeded to make experiments, and had found that they produced no result by inoculation with blood or with cerebro-spinal fluid from cases of the disease; but in the peritoneal fluid they found certain organisms, and were able to produce the disease by inoculation with this fluid. From these experiments, they came to the conclusion that louping-ill was due to a specific bacillus taken up from the soil into the intestines, and passing into the peritoneal cavity.

In the study of braxy, they had before them Nielsen's discovery of the specific bacillus of this disease. Braxy and louping-ill were due to organisms of the same nature, and having the same habitat.

No curative treatment was of any real use against these

diseases; nor had segregation any good effect, because the diseases were so widespread. Experiments therefore were made to immunise the sheep. At first, use was made of a hypodermic injection of attenuated cultures of the organism; later, use was made of cultures administered as a drench. For one example of the good result of this latter method, Professor Hamilton said that a proprietor on the west coast of Scotland, having a stock of 420 first-year sheep, drenched 400 of them. Out of the 20 left to Nature, he lost 19; out of the 400, he lost only 30 from all causes, and very few of them, if any, from braxy. "That has been my invariable experience. If one has a chance of comparing two lots like these, in the one case the number of braxy cases you get is something appalling." These diseases of sheep were a very complex matter, and there was still a very great deal to be learned; but, with certain exceptions here and there on particular farms and in particular districts, the returns, so far as braxy was concerned, had shown that the mortality in the drenched sheep was very considerably lower than in the undrenched.

The witness was asked, "Do you find that farmers, generally, willingly lend themselves to this attempt to immunise their sheep by drenching?" And he answered, "I have not had the slightest difficulty; I have had every encouragement from farmers: I say it to their credit that they behaved extremely well under the circumstances."

He was also asked: "It has been suggested here, by various witnesses, that experiments on animals with a view to extend knowledge with regard to human or animal disease ought to be absolutely prohibited by law. What do you think would be the general feeling of agriculturists in Scotland with regard to such a proposal?" He answered: "I would not like to speak for the agricultural community; but from conversation and wide experience

that I have had with intelligent farmers, I think it would be perfectly disastrous: it would raise a *furore* among the intelligent agricultural population, were you to attempt anything of the kind. That is my impression."

MR. F. HOBDAY, F.R.C.V.S., F.R.S.E., *November 19, 1907*

Mr. Hobday, sometime Professor of Therapeutics in the Royal Veterinary College, and Examiner in Therapeutics at the Royal College of Veterinary Surgeons, gave evidence in veterinary surgery. For a number of years he had made a special study of anæsthetics in veterinary surgery, and had made many experiments to that end, under the Act. He treated many thousands of animals annually. He had studied anæsthetics mostly on dogs and cats, but also on pigs and lambs. He had kept records of his first 1,200 consecutive cases of dogs anæsthetised in veterinary practice; he had had many thousands of cases since that time. Out of his 1,200 consecutive cases, which he recorded ten or twelve years ago, five died of the anæsthetic. "But I want particularly to say here, that these patients were taken as they came." The great majority of these dogs were out-patients at the Royal Veterinary College, "and we had not an opportunity to prepare them as one would do in ordinary practice with a patient brought to a veterinary surgeon for anæsthesia, mainly because we had not accommodation for taking in a number of animals to prepare them previously; partly because the owners themselves are usually very fond of their animals, and will not allow them away from home longer than they can help; and partly because, with a busy clinique, such as we have at the College, we have not time to do anything more than one considers to be absolutely necessary in the way of preparation." His later experience of anæsthetics for dogs was even more favourable.

Asked whether he agreed with Sir W. Thornley Stoker's statement, that he should think it was generally impossible to keep a dog alive for two hours under full anæsthesia, Mr. Hobday answered, "Absolutely, no." He did not think, he knew it was erroneous. It was the idea years ago, but it was erroneous. He would prove that to anybody. He was so satisfied of it, that he could not understand any one, who had had any experience at all, making the opposite assertion. He knew that it had been a common impression, because he had many thousands of people to deal with in the course of a year, and a very great number of them were ladies who had pet dogs, who had this impression, or had had this impression. He had kept a dog under anæsthesia for five hours. The anæsthetic, in his practice, was usually administered by a specially trained nurse.

Asked whether his experience bore out Sir W. Thornley Stoker's statement that dogs are terrified of chloroform, he answered: "No, it does not. A dog will struggle against anything being put on its nose, but it is only struggling against a foreign body. It struggles to get away, as every dog will. I purposely did something to test that question, a little while ago, in front of several gentlemen who had an idea that a dog would show a great deal of terror. They were gentlemen connected with a certain Society. I asked two of them to come one afternoon, to see that what they had thought was not really the case. I did two operations under chloroform; those were all that I had that afternoon; and I took a number of dogs out of the hospital kennels, and put them on the operating-table with the hobbles. They were put there gently, as we always do put them, and when a dog is put on the operating-table with the hobbles, if it is soothed and talked to gently it does not show excessive fear."

Asked about the healing of wounds in dogs, and the use of bandages, he answered: "I never bandage wounds. Many thousands of dogs have had operative wounds made on them, and have never been bandaged; and I have never bandaged an antiseptic surgical wound on a dog but once during the last six years, and that time I regretted it. I did it at the owner's request, and I regretted it. I do not always get a healing by first intention, but in by far the majority of cases I do. That means that I have no irritation there, and the dog has not licked his sutures out. The operations are done antiseptically, and the wound afterwards is dried properly, with ether and various things. Time is taken over all this: and the wound is afterwards dressed with iodoform colloid, or some such preparation, in order to hermetically seal it from the exterior."

Asked whether the statement were true that extensive surgical wounds on the lower animals must necessarily be painful when the animal comes out of the anaesthesia, he answered: "My own experience is, that the statement is absolutely untrue. If my wound was painful, I should expect, from my knowledge of the dog, or even of the monkey—for I have also had experience of monkeys—that the animal would immediately go for those sutures and lick them out, or, at all events, lick the wound continually. That is evidence of irritation in the dog, but I do not find that as a rule. By far the greater majority of my wounds heal by primary union, and apparently without pain. The dog is simply put into a cage and given over to the nurse, until it is out of the chloroform; and when once it is out of the chloroform, it is simply put into a kennel like other dogs."

Asked about distemper, he answered that it would be impossible to investigate distemper without experiments on

dogs. To let a few dogs, comparatively, have distemper, whether given artificially or whether given naturally, would be a much better thing than to let hundreds of thousands die every year from this scourge, as they do at the present time.

Asked about anæsthesia for horses, he said that chloroform could safely be given to a horse.

Asked about the most humane method of destroying a pet animal, he said that prussic acid was the quickest, but in his opinion anæsthesia was the most humane. Morphia required a very large dose to be fatal. He used morphia largely, for stopping pain. He had seldom used chloral for dogs; and had not used urethane. He did not use ether much, because of the after-sickness in dogs. Asked further about morphia, he said that he used it for minor operations sometimes, but not for major operations. For a tracheotomy he would as a rule use cocaine. Personally, he did not think it would be easy to give a lethal dose of morphia to a dog. He had known a dog survive a dose of twenty grains of morphia; but the dog was absolutely dead to the world, and was unconscious to pain.

IX

ANÆSTHETICS USED IN EXPERIMENTS ON ANIMALS

MR. G. D. THANE, LL.D., *November 7, 14, 21, 1906*

DR. THANE, Inspector for Great Britain under the Act since 1899, referring to the use of anæsthetics, said: "In my experience, anæsthesia is practised in an experimental laboratory as a matter of routine—not in a disparaging sense, but as an essential part of the procedure, a matter of course. In fact, anæsthesia and asepsis are carried out in the laboratory, to the best of my knowledge and belief, as strictly as in the operating theatre of the hospital."

He was asked: "Is it your experience that dogs and monkeys are terrorised when they are taken out to be anæsthetised and operated upon?" He answered: "Not that dogs are. Monkeys are, of course, always very shy creatures: they are very frightened, if you even go and look at them: they often fly away—fly to the corner of the cage and knock about. Monkeys are very fearsome animals." He was quite sure that he had seen animals effectually kept under complete anæsthesia for long periods of time; he was absolutely confident that the anæsthesia was always complete. Asked about morphia used as an anæsthetic, he answered: "Morphia is very rarely used as an anæsthetic alone; that is quite certain. It can be ad-

ministered so as to secure complete anæsthesia—there is no question about it; but you probably would have to give a fatal dose.” He pointed out to the Commission that a known proportion of anæsthetic vapour could be delivered per minute to an animal according to the weight of the animal, so as to ensure continuous and complete anæsthesia.

SIR JAMES RUSSELL, LL.D., *November 7, 1906*

Sir James Russell, Inspector for Scotland and the Northern Counties since 1890, was asked about the giving of anæsthetics to animals. He said, speaking of recovery from anæsthesia, “I have seen an animal drunk from the anæsthetic for an hour or two after an operation; and in another case, after a severe operation, I have seen the animal run about within an hour. I have mentioned one case in my *précis*, in which I came upon a licensee taking away part of the brain from a cat. After the operation was finished, and the wound dressed with collodion dressing, the animal was wrapped in flannel and put on the floor before the fire, to recover from the chloroform which had been given to it. I went round the rest of the premises, and came back again to the room where the operation was performed; and when I came back, I found the cat dressing its fur, and then I saw it walk across the room to a saucer of milk.”

Asked about experiments under licence alone, he said: “Those experiments are mostly blood-pressure for testing the effects of drugs, with tubes attached to the arteries, and artificial respiration in some cases. Those are generally very prolonged: from an hour, say, to three or four hours. I have watched one experiment myself, which lasted from a quarter to ten in the morning to half-past three in the afternoon, when I saw the animal killed. I always test

the anæsthesia myself, by testing the reflexes. I do not think that the licensees would dare to do in a hospital what they do in laboratories in the way of giving anæsthesia. I have frequently seen animals they have killed before beginning the operation, owing to pushing the anæsthesia too far. I saw one last week. I went into a laboratory where I found two licensees trying, by artificial respiration, to revive a monkey not yet touched with the knife: they had given it too much ether. Then they had to use another monkey, and I saw the experiment done on the second monkey from beginning to end, and even with it I thought they pushed the ether very far, though the monkey recovered, and I saw it two or three days after and it seemed all right. On several occasions I have found licensees with dead animals in their hands before they had begun experimenting, owing to overdosing with chloroform or ether."

The following questions were put and answered:—

Q. You have spoken of your own observation of long and severe operations.—*A.* Yes.

Q. And you are quite satisfied that the animals had, at the least, a full dose of anæsthetics?—*A.* Yes; a very full dose.

Q. Do you know—perhaps you will leave it to some one else to answer—whether it is not a difficult matter in the case of a dog, for instance, to limit the exact amount which will kill and which will not kill: whether it is not a very difficult thing to keep a dog just on the border-line between life and death?—*A.* I would not say so. The result of my personal observation of experiments on dogs has been that they were very fully under; and I do not think they are so easily killed. I have no reason to believe that they are more easily killed than, say, rabbits, by overdosing with anæsthetics.

Q. You have said that they were as completely anæsthetised as a human being would be, and as carefully?—
A. Generally more so, those I have seen.

Q. Then, may I ask you, is the dog under those circumstances loose, or strapped down?—*A.* He is generally tied up on a warm trough, when it is a demonstration that goes on for a long time; but, for an ordinary minor operation that lasts a few minutes, he would not be tied. It depends upon what is going to be done to him. I have never seen an experiment under a licence which was to last for hours without the dog being tied up and profoundly under anæsthesia. For a short surgical operation on a dog, it would not be tied. May I explain how I have seen it done in the case of an experiment under license alone? I saw a dog put into a box; then a mixture of chloroform and air was pumped through this box until the dog fell down insensible; then the dog was lifted up and put on a table, and ether applied with cotton-wool to the nose till it was completely under; and then they tied it up and went through the long process of a blood-pressure experiment.

Q. Why was the tying necessary?—*A.* I suppose to keep it steady in position.

Q. But would it not be to guard against its struggling on regaining partial sensibility?—*A.* No. When they put a number of delicate glass tubes into the carotid artery and the trachea or the veins, you can see that if the dog were touched during the experiment, and were to roll the least degree to one side or the other and break the tubes, it would lose some hours' work. And then the recording drum that they have has to be adjusted to the hundredth of an inch or less. Therefore, the very faintest movement, even a fraction of an inch, by the animal or people working about it, would cause trouble.

Asked about the pithing of frogs, he said that there were

some experiments where they were not pithed, so far as he had seen, but the great majority were pithed.

He was asked: "Do you think it is perfectly possible to keep a dog under complete anæsthesia for an hour and a half or longer?" and he answered: "I do not see any difficulty in it." He had never seen the animals struggle; they were far too deeply under for that. "And, further, if it is an operation before a class, say in the Edinburgh University, where the Professor has two or three hundred critics in front of him, I think he would be a very bold man, even if he had the heart to do such a thing, who would venture to show an experiment where the anæsthesia was not complete." He was asked whether it was within his knowledge that much improvement had taken place in the administration of anæsthetics, and that we had more control over anæsthetics now than we had thirty years ago. He answered: "Certainly; and precautions are taken now which were not taken then: for example, I mentioned the mixing of alcohol with chloroform, and the use of atropine. What I have seen, as I have said already, is, that experimenters nearly always push the anæsthesia more boldly than they would in human patients, naturally feeling that the death of a human being would be a very serious matter, and of an animal less serious; and so they can make sure that they have the animal thoroughly under, by pushing the anæsthesia."

SIR WILLIAM THORNLEY STOKER, *November 14, 1906*

Sir William Thornley Stoker, M.D., President of the Royal Academy of Medicine in Ireland, and Inspector for Ireland under the Act since 1879, read a memorandum which he had furnished to the Chief Secretary for Ireland some time ago. In this memorandum he expressed a fear

that anæsthesia, particularly in the case of dogs, was not always pushed to a sufficient extent, as these animals often died from the effects of the anæsthetic if given to a full extent. Asked as to his personal experience regarding humanity and carefulness over experiments in Ireland, he answered: "I think that, so far as the letter of the Act is concerned, it has been administered and observed and practised with a great deal of humanity in Ireland." He had never found anything irregular, or to complain of. He thought that experiments on dogs and monkeys ought to be allowed with the greatest possible reserve. "The amount of terror that a dog feels, even in being put under chloroform, is rather painful to witness. I have never myself seen monkeys experimented upon, but I have been told by reliable observers and physiologists who have, that a monkey evidences the most acute sense of fear when it is brought into the room where it is going to be operated on, and shows a great degree of terror. Professor Purser, lately Professor of Physiology in the University of Dublin, is my informant on that point." Sir Thornley Stoker said that he should think it was generally impossible to keep a dog alive for two hours under full anæsthesia, or for one hour, because it was so susceptible to death from chloroform that the anæsthesia could not be completely maintained throughout a long experiment without killing the dog.

Further examined, he said that he had never seen experiments done in illustration of lectures. Very few demonstrations were given in Ireland.

Further examined, he said that he had not taken any trouble to keep himself informed with regard to the investigations which had been carried on, in recent years, as to how dogs might best be anæsthetised. He had never himself seen any experiment under the Act more than a mere inoculation. He did not think there were any abuses

existing in Ireland. He did not think there was any concealment, or anything that a dozen more Inspectors could find out.

PROFESSOR STARLING, *December 12, 19, 20, 1906*

For Professor Starling's general evidence in physiology, see Chapter III. He was also examined as to the anæsthetics used in experiments on animals. "The usual thing we do," he said, "is to give the animal, half an hour before the experiment, a hypodermic injection of morphia, of about a quarter of a grain—from a quarter to a third. The effect of that is, that the dog becomes sleepy and stupid, and then sometimes it will lie down quietly; and if it is very sleepy you can put a mask over its nose containing the chloroform, alcohol, and ether mixture, which it takes quite quietly. If, at the time one wants to begin the operation, the animal is not fully under the influence of morphia—if it still seems restless—it is put in a box, and there it has some wool saturated with the A.C.E. (alcohol, chloroform, and ether) mixture put in the box. The air gradually gets saturated, the dog gets more and more sleepy, and finally subsides in the bottom of the box. Then we take it out, fix it up on the table, and continue the administration of the A.C.E. mixture through the mask." The dog would have to be fixed, because it could not lie on its back. He described the difference between conscious and unconscious movement. He pointed out that the sense of pain is one of the earliest senses to disappear under anaesthesia. It disappears before the sense of touch disappears; and the sense of touch disappears before the power of movement.

The administration of anaesthetics was a routine practice, just as it would be in the operating theatre. Nobody ever

thought of doing any cutting operation without thorough anæsthesia. "I know practically every physiologist in England; and there are very few whom I have not seen doing experiments, at one time or another. And the intention of the experimenter in each case is the same as my intention would be; that is to say, to prevent, throughout the whole experiment, the animal from feeling pain—to make the whole thing painless."

Professor Starling went on to speak of the doses of morphia, chloral, and urethane given in cases where these drugs were used for anæsthesia. "In man," he said, "we give from 5 to 20 grains of chloral hydrate, that is, about 0·02 gramme per kilo. In the animal, we give half a gramme per kilo; that is, fifty times as much, and then we get complete anæsthesia. Morphia is generally used as an adjunct to chloroform and ether. When we give morphia as an adjunct to chloroform or ether, we give from one sixth to a quarter of a grain; when we give it as an anæsthetic, we give from one and a half up to fifteen grains, according to the size of the animal; that is to say, a dose that is practically fatal: in most cases the dog would die if it were kept under that anæsthetic. Sometimes, in one or two cases, dogs do recover from the average amount, if they are kept perfectly warm; but in nearly all cases they die of the dose. It is a fatal dose, and the condition of the animal is the same as in the case of opium poisoning in man. The question of complete anæsthesia will, in each case, be a question of the dose, whether you are dealing with chloroform, or whether you are dealing with morphia. Morphia is a complete anæsthetic, if it is given in large enough doses."

SIR T. LAUDER BRUNTON, BART., M.D., F.R.S.,
April 24, 1907

For other evidence given by Sir Lauder Brunton, see Chapter IV. With reference to anæsthetics given to animals, he was asked the following question: "We have been told here repeatedly that morphia is not an anæsthetic: that depends, of course, upon the quantity. We have been told, also, that chloral is not an anæsthetic: that also depends upon the quantity. These animals receive poisonous doses, in order to completely narcotise them?" He answered: "Yes; and as to the statements that chloral and opium or morphia are not narcotics, and do not remove pain, there is no other word for it, it is simply a lie; you may as well say that chloroform does not remove pain. If you give any animal a sufficiently large dose of chloral or opium, you so completely abolish sensibility that there is nothing you can do that will awaken its sensibility; you might burn it alive, you might throw it on the coals, and it would remain unmoved. The animal is as senseless as a piece of board."

Asked whether dogs were more difficult to keep under chloroform than cats or other animals, he said: "Certainly not." He had had experience of chloroform given to a vast number of dogs. He had entirely convinced himself that they had been under complete anæsthesia: as complete as any patient could possibly be under on the operating table.

SIR HENRY MORRIS, Bt., *May 8, 1907*

For Sir Henry Morris's general evidence in surgery, see Chapter V. With reference to anæsthetics, he was asked: "Do you think that an animal, a dog or a cat, which receives a poisonous dose of morphia or opium, is in

a condition to feel pain up to the time that death occurs?" He answered: "No, I do not think so. They suffer no pain." He was then asked: "We have been told here by several witnesses that opium is not an anæsthetic, and therefore, even when a poisonous dose is given, the animal is probably suffering tortures till death occurs?" He answered: "That is not so." Later, he was asked: "You are absolutely satisfied in your own mind that complete anæsthesia is always aimed at most carefully, and in your opinion secured?" He answered: "Yes, I do think so; and certainly I have always seen it." He was of opinion that even a very severe and prolonged operation, lasting for an hour or an hour and a half, might be performed on an animal, under morphia alone, without the animal feeling any pain.

DR. DUDLEY BUXTON, M.D., *July 17, 1907*

Dr. Dudley Buxton, M.D., Anæsthetist and Lecturer on Anæsthetics in University College Hospital, and sometime President of the Society of Anæsthetists, gave evidence on the general action of anæsthetics. He said that the use and the action of anæsthetics had been ascertained, largely, by means of experiments upon living animals—by testing them upon animals. There was no difference between their action on man and their action on animals. He had seen a large number of experiments on animals, and had very many times given anæsthetics to them. He had never seen any cruelty; he had seen reflex movements, but had never seen conscious movements. It was certainly not his experience that dogs were particularly difficult to keep under anæsthetics: they could be kept under them for several hours if you understood your business.

He distinguished between "light anæsthesia" and "incomplete anæsthesia." Later, the following questions were put and answered:

Q. Has your attention been called at all to some experiments that were performed by Dr. Crile on surgical shock?

—*A.* I am aware of Crile's experiments in book form.

Q. May I put this to you? Dr. Crile, in his work on surgical shock, mentions certain experiments which he performed under what he called incomplete anæsthesia. He was then asked whether those animals suffered any pain or not; and his reply was: "Certainly not; they suffered no pain," although the term that he used was incomplete anæsthesia. What should you say to that?—*A.* I should think that probably he used the expression, incomplete anæsthesia, somewhat loosely.

Q. In the sense in which you would use light anæsthesia?

—*A.* Yes, I should imagine so. Even that would depend very much upon how near complete anæsthesia his incomplete anæsthesia was. He probably meant that he had not got the condition where the animal had completely lost the conjunctival reflex, and the eyeball had not become fixed, and so on.

Q. But, nevertheless, it might have reached the stage that you call complete surgical anæsthesia.—*A.* Yes; but not if it was incomplete anæsthesia.

Q. He called it incomplete anæsthesia, but said that the animal suffered no pain. Could those statements be reconciled if he was using the term in the same sense that you do?—*A.* I do not think he could.

Q. You think he was using the term incomplete anæsthesia (if his statement is correct that the animal suffered no pain) in a different sense from that in which you use it?—*A.* Yes.

Asked about the "Brown Dog," he said that he had no doubt whatever that the animal was completely anæsthetised.

The following questions were put and answered:

Q. In physiological experiments on dogs, of which you

have seen a great many, they are all strapped down first of all, are they not, before the chloroform is administered?—*A.* No, certainly not.

Q. Is there not some difficulty in administering it? The dogs struggle against it?—*A.* They are in a box; you can put them into a box.

Q. The dog struggles, I presume, to get out of the box?—*A.* No; it generally lies down and goes to sleep.

Q. Not at first. In the early stages, does it not struggle, or bite the box?—*A.* As a rule, my experience is that the dog turns round once or twice and goes to sleep.

Q. And then is it strapped down?—*A.* Yes, when it is unconscious, and tied on a board.

Q. That, you say, is to prevent reflex action?—*A.* No; I did not say anything of the sort.

Q. What is the object?—*A.* To keep them in the position required for the operation.

Dr. Dudley Buxton added that in many of the operations of surgery the patient had to be secured in position with straps to keep him in that position during the operation.

PROFESSOR GOTCH, *October 30, 1907*

Professor Gotch (see also Chapter III) said that, so far as his belief went, it was a certainty that complete anæsthesia could be obtained in animals for a very great length of time. He had been under a severe operation himself, which lasted two and a half hours; and he thought the most difficult animal to maintain in a prolonged condition of anæsthesia would be man. Asked about chloral, used in a lethal dose as an anæsthetic for rabbits, he answered that the rabbit would live for two hours in a state of absolute and profound anæsthesia, gradually deepening into death. Asked about urethane, used in a similar way, he said that he considered it to be as efficient for producing anæsthesia as

a respirable vapour ; and he preferred it because the dose, being absorbed from the alimentary canal, must go on gradually increasing to a certain point.

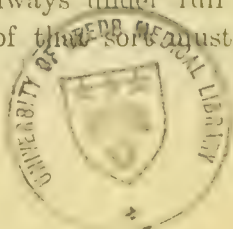
PROFESSOR SCHÄFER, *June 12, 1907*

Professor Schäfer (see Chapter III), in the course of his evidence in physiology, was asked the following question : "I should like your opinion, as regards both your own experiments and any experiments that you have seen performed by others, as to whether or not complete anæsthesia is always secured." He answered : "In all experiments which come under licence,¹ complete anæsthesia is secured during the whole period of the experiment, however many hours it lasts. I do not mean to say that an animal will never make a movement when it is under anæsthetics : of course an animal will, as a man will. But the object of the experimenter is to keep the animal under anæsthesia ; his objects are likely to be entirely frustrated if the animal moves ; and, although the condition of anæsthesia may be more or less complete, the amount of anæsthesia used in physiological laboratories is in almost every case vastly deeper than that which is used in the operating theatre. It does not matter very much to us whether the animal dies of anæsthesia or not. We are much more inclined to push the anæsthetic, because it is of the utmost importance that there should be no movements whatever, inasmuch as the least movement of the animal will entirely upset the delicate apparatus that we have to use. And the suggestion that we knowingly, or even unknowingly, allow the animal to come out of the anæsthesia, is on the face of it an absurd suggestion, because it would absolutely defeat the object of our ex-

¹ *I.e.* all experiments after which the animal is killed under the anæsthetic, and not allowed to be kept for subsequent observation.

periment." There was no grain of foundation for the suggestion that the anæsthetics in this country were not given fully and carefully, and so as to ensure complete insensibility to pain. Asked about the giving of chloroform to dogs, he said that a very small dose of atropin, before the administration of the chloroform, entirely abolished the inhibitory action of the chloroform, through the vagus nerve, on the dog's heart. "In my experience, dogs are much more easy to anæsthetise than any other animal. One is much more liable to lose a rabbit than a dog with chloroform, especially if you give the dog a prior dose of atropin."

He was asked: "Have you observed, in any operations that you have seen performed, either when you were a student or afterwards, any indifference or carelessness on the part of operators as regards anæsthetics?" He answered: "I have never observed it. And I would not confine that answer to this country. I have witnessed a great many operations in Germany and other countries; and the statements which fly about regarding the callousness of foreign physiologists to the sufferings of animals are wild, and quite unconfirmed by anything which I have seen. I myself worked for three months in Professor Ludwig's laboratory at Leipzie, where Dr. Gaskell was also working at the same time; and the system of anæsthesia employed was a very delicate one indeed, and had the result of rendering the animal absolutely quiescent during the whole of the experiment. That is in a country where it was not compulsory, and a country which has been specially vilified with regard to this subject. And I have seen at congresses of physiologists large numbers of operations performed, always under full anæsthesia. So that I think statements of that sort must be received very carefully indeed."



DR. WALLER, *November 27, 1907*

Dr. A. D. Waller, M.D., LL.D., F.R.S., Director of the Physiological Laboratory of the University of London, gave evidence as to certain statements in the "Shambles of Science." He read to the Commission part of a lecture which he had given, at the University of London, with reference to anti-vivisection generally:

"... The naked details of even a properly conducted vivisection, ignorantly considered, as must be the case if they are considered at all by an unprofessional person, appear revolting: so do the details of a surgical operation, or of the slaughter of an animal for food, or of the proceedings in the dissecting-room and the *post-mortem* room. . . .

"Physiologists, when vivisection is necessary, do not take either pleasure or pain in its naked details; and they systematically take due precautions to fully anaesthetise animals required for experiment. The incredible motives sometimes attributed to physiologists, by well-meaning people, and the unlimited adjectives and substantives by which they are held up to public reprobation, can only be left to the antidote of their own excess. It is really labour lost to be constantly pleading 'Not guilty' to this, that, or the other quite outrageous statement; we can only wonder that well-meaning people can so quietly harbour such infamous thoughts. . . .

"When one attempts to realise what kind of images must hold possession of an ignorant, prejudiced, and sensitive person, man or woman, who has seen without understanding, he ceases to wonder at bad language; yet what can he hope to say that shall reach the mind of the deluded fanatics whose pity has been fanned to hatred by agitators? And can it be expected of us, that we should say anything at all to persons who can employ sensational literature to poison the wells of human sympathy? In this laboratory—and no doubt in others—our very first concern is to

administer anæsthetics properly when anæsthetics are required. And I make the deliberate statement, that animals in this laboratory are anæsthetised with as great certainty and accuracy as are the patients in any hospital in the United Kingdom."

Dr. Waller stated that he had made a special subject of anæsthetics: he had worked at it a great deal in the last fifteen years. He stated, against certain statements in the "Shambles of Science," that in every case the animal was under complete anæsthesia. "One does not mention it on every occasion. At every turn, the animal is anæsthetised; but it may often happen that one does not mention it." Asked, whether the use of the word "light" or "deep" was any indication whatever of any degree of pain, he answered that it was none. He was not aware of any laboratory in England where tracheotomy would be performed without an anæsthetic. Asked again as to the meaning of "light anæsthesia," he answered: "In the state of light anæsthesia, one knows that the first movements, in the case of a man, to disappear, are the purposive movements, the obviously purposive movements; and one knows that when that happens there is no sensation; and one judges from the character of the movements that the animal is in an analogous state, having no sensation. Only, as a matter of fact, my practice, and I think the practice of most physiologists, is to go as far as the conjunctival reflex." Once that reflex was abolished, there would be a very considerable interval of time, even if no more chloroform were given, before any sensation of pain would ever come back. Besides, the dosage of chloroform-vapour, the proportion of chloroform-vapour to air, would be known. "You know that you can keep your animal anæsthetised; that if you have brought it down by 2 per cent., you can keep it there at

1½ per cent., 1 per cent., and ½ per cent., as time goes on. I have had an animal as long as twelve hours under anæsthetics, and at that time it wants only a very low percentage indeed to keep it under."

CURARE

Curare is not allowed to be used during any operation under the Act, unless the animal be under the influence of an anæsthetic. It is used only in a very small minority of experiments, and is never used instead of an anæsthetic.

Though curare, in small doses, only abolishes motion, yet, in large doses, it also abolishes sensation. Evidence for this belief is afforded not only by Mr. White's case of arrow-poisoning, and by Schiff's experiment on the exclusion of one limb of a frog from the action of the curare, but by the character of the drug.

DR. DIXON, *December 4, 1907*

"It is not an isolated drug, having this peculiar action all to itself. There are lots of other drugs having the same type of action. It is, perhaps, the one which has the most characteristic action on the motor-nerve endings; but there would be no difficulty in picking out a whole host of others that do the same. All this group of drugs paralyses the nerve cells, the brain; and every one of them paralyses the motor-nerve endings; and they may all cause convulsions by acting on the spinal cord. Those three facts apply to all of them. Some members of the group have one action well defined, and others another. Thus, nicotine first paralyses the nerve cells, and later the motor-nerve endings: whilst hemlock (conium) paralyses the nerve cells and nerve endings almost together. Curare first paralyses the motor-nerve endings, and later the nerve cells: whilst hemlock—

the poison that killed Socrates—paralyses the nerve cells and nerve endings, roughly, about the same time. I picked those three examples from a group, to show the various stages: how one produces its action at one time, and another at another time.

“I mention this to show that even curare given alone is a complete anæsthetic, if enough is given: although we in England, conducting experiments, assume that curare has no action on the nerve cells, and always give enough of some other anæsthetic to completely paralyse the brain. Of course, Claude Bernard really started this idea that curare acts on the motor-nerve endings, and not on the sensory-nerve endings, or cells. But Claude Bernard's experiments only apply to the spinal cord: he did not prove anything else at all; all that he showed was, that the sensory cells in the spinal cord are not paralysed by curare. That was all his experiments meant.”

PROFESSOR STARLING

“Curare is used for poisoning arrows by the Indians, and it is brought by them into commerce in gourds. It is now becoming extremely difficult to get curare, and it is becoming more and more impure, more and more poisonous in its effects, so that it is being used as little as possible. A case in which one must use curare is, where one is exciting, say, a nerve going to the arm. We know that this nerve going to the arm contains fibres which will cause contraction of muscles, and which will also cause contraction of blood vessels. If we want to get a contraction of blood vessels, we cannot record these, or see whether they contract, if the muscles are contracting at the same time. So we should give curare, which would paralyse the nerves ending in the muscles, but would not paralyse the nerves ending in the blood vessels; so that, after curare, if we stimulate this

nerve *here*" (*describing the same*), "the blood vessels would contract alone.¹ It is under that sort of condition that one has to give curare."

Professor Starling was not satisfied that curare could abolish consciousness: anyhow, it was never used as an anæsthetic, it was assumed to be without anæsthetic effects; it was therefore only used in association with anæsthetics. The absolute prohibition of curare would interfere seriously with some of the most important classes of experiments. "I do not see why, because in ignorance people object to the use of a drug by competent persons, that should be any guide either to our lawgivers or to the findings of this Commission. Curare is a drug which is used for specific purposes: it is not used instead of an anæsthetic; its use does not interfere with the carrying out of the law; its use is essential to certain parts of physiology. I think that every precaution is taken that the use of curare is only in the hands of people who are competent to deal with it. For my own work I use curare very little."

Later, the following questions were put and answered:

Q. Now, one question with regard to curare. You told Sir Mackenzie Chalmers a great deal, but I want to ask you something more about it. When an animal has had an anæsthetic administered, and a dose of curare also, if the anæsthetic passed off the animal would still be unable to move, or to show any sign of suffering?—*A.* Yes.

Q. Are there any means, other than the cries or struggles of the animal, by which you can tell whether the anæsthetic is passing off?—*A.* Yes; you can tell it by the blood pressure. Struggles have also what we may call their visceral side. This activity of the muscles of the

¹ It may be well to note that the stimulation of a nerve in the direction away from the brain, even without anæsthetics, does not give rise to pain.

body is associated with activity of the centres which govern the blood vessels ; and when one is working without curare, one notices that the pressure goes up, and then, if one does not attend to it, after that comes a little movement, and you give more anæsthetic.

Q. So that the presence of curare does not prevent your knowing whether the anæsthesia is complete or not?—*A.* No ; it would make it more difficult, but you have that clue. What one does, of course, is to ensure the complete anæsthesia, and continue that anæsthesia during the curare—continue the same amount.

Q. Is curare ever given under Certificate B. in cases of animals that are to recover from the anæsthesia?—*A.* Never.

Q. Therefore, every animal that has curare is, so to speak, bound to die under the anæsthetic?—*A.* Yes.

Asked whether he had the faintest reason to suppose that curare intensifies the capacity for sensation, as Mrs. Cook had stated, Professor Starling said that there was absolutely no evidence to support such a statement. “The evidence that we have, which I have already said is incomplete, is in the opposite direction—namely, that it would diminish the amount of pain, even if it does not absolutely abolish it. There is no physiological evidence that it intensifies the sensory impressions.” Asked further as to the possibility of pain during a prolonged experiment under anæsthesia *plus* curare, the anæsthetic being given by artificial respiration through a tracheotomy tube, he answered : “The tendency, of course, of anæsthesia is not to become less. If you continue the administration of a certain dosage of chloroform, the anæsthesia gets deeper and deeper : it does not get less and less, but deeper and deeper. In those cases where you are going to give curare, you have this volatile anæsthesia automatically being de-

livered by pumping, and it continues; you give curare, and that continues until the animal is dead. If anything happens, it will be a continual deepening of the anæsthesia, not a recovery from the anæsthesia. It cannot stop. Curare is given after a state of anæsthesia has been established. The air is being pumped in, charged with a certain amount of vapour, and the whole thing is regular. I know it; it is not a question of belief. I know that an animal, with that dose of anæsthetic, is fully under the influence of the anæsthetic."

PROFESSOR CUSHNY, *February 26, 1907*

"I do not think curare is much used anywhere. To tell the truth, it is very difficult to get; and the reason why I refuse to give any definite statements as to curare is, that it is so indefinite. We have not the same curare that we could have got thirty or forty years ago. As a matter of fact, at the present time much of the curare that we get fails to paralyse muscles or anything. I tried to get it in a number of places a few years ago, and I could not get any curare that would paralyse the muscles at all. What is called curare is very often quite inactive."

SIR T. LAUDER BRUNTON, *April 24, 1907*

"Any experiments I made with curare would be previously to the existing law: previously to 1876. I do not recollect when I last used curare. There are certain cases which one can conceive, where the employment of curare might be very useful."

PROFESSOR SCHÄFER, *June 12, 1907*

"I have not used curare at all for years. I have not been engaged in any experiments which require the use of curare."

SIR RICHARD DOUGLAS POWELL, *March 5, 1907*

"Curare is very expensive, and very difficult to obtain—very difficult indeed; and I cannot conceive of any man using curare for any other purpose than to prevent reflex muscular movements which would interfere with his experiments under an anæsthetic. To substitute it for an anæsthetic would be a piece of clumsy extravagance which I cannot imagine any sensible man making use of."

SIR JAMES RUSSELL, *November 7, 1906*

"I have not seen curare used for years. I did once see it used in a blood-pressure experiment about fifteen years ago. In that case the anæsthetic was a very heavy dose of urethane, administered before the experiment began, and the animal was heavily narcotised. I happened to see that experiment from beginning to end."

PROFESSOR GOTCH, *October 30, 1907*

"I have had very little experience of curare. I do not use it for the blood-pressure experiments that I show. I have not used it for the research experiments that I have done upon the nervous system."

PROFESSOR LANGLEY, *November 12, 1907*

"I have had experience of the use of curare for particular experiments; and, having obtained a knowledge of the way to administer anæsthetics, I am confident that there is no difficulty in keeping up the anæsthesia while curare is given. The operator must start with complete anæsthesia; and he must know, from past experience, the amount of the anæsthetic which it is necessary to give in order to maintain it. For instance, with chloroform, the

chloroform is given at the intervals and in such doses as his previous experience has shown will maintain complete anæsthesia."

DR. WALLER, *November 27, 1907*

Dr. Waller stated that he had never known curare administered without an anæsthetic causing complete insensibility to pain. One could be certain that the animal was completely anæsthetised. "It is immobile—so is a plant immobile—but I know, if you give it 2 per cent. of chloroform vapour, it is of necessity under the influence of the chloroform vapour. If artificial respiration is going on with the percentage of chloroform vapour, I should be satisfied. I know that I am on the safe side as regards pain to the animal, and on the unsafe side as regards the life of the animal. I am more liable to lose the animal than to let it out of anæsthesia."

X

ANTI-VIVISECTION EVIDENCE

MRS. K. COOK, *November 21 and 28, 1906*

MRS. K. COOK ("Mabel Collins"), authoress and journalist, and Chairman of the Executive Committee of the Parliamentary Association for the Abolition of Vivisection, after alluding to Metchnikoff's work, referred to certain published experiments made between 1879 and 1900. She said that the antiseptic treatment does not in any way lessen the pain of a wound: "I will point to the well-known instance of the King's operation, when Sir Frederick Treves treated the wound antiseptically: and the papers daily stated that he suffered greatly from the wound, on the authority of his medical attendants."¹

Mrs. Cook proceeded to refer to other published experiments in physiology and pathology. She was then asked about an "open letter" which had been sent by her Committee to the Commissioners. She said that it had been drawn up by Dr. Arabella Kenealy. She was not in favour of mere restriction of experiments on animals; she would prefer the repeal of the present Act. She stated

¹ It will be remembered that H.M. the late King underwent operation for a large abscess. The wound had to be kept open, to ensure drainage; and for several days His Majesty suffered pain from the necessary changing of the dressings.

that her Society was formed in 1902, and had about 200 members.¹

Asked about experiments on animals to find a cure for snake-bite in India, and a preventive treatment against plague in India, she said that she thought them unjustifiable. Asked whether she could give any instances, within the last fifteen years, since she first was connected with the subject of vivisection, where operative experiments had been allowed without anæsthetics, she said that she felt sure she could supply such instances if the Commission would give her time to look them up.² Asked whether she knew of any proceedings ever taken, either by her Society, or by any other person, on account of experiments made in unlicensed places, she said "No." Asked whether she objected to the testing, on a guinea-pig, of milk suspected of tubercle, she said "Yes." She was not prepared to admit that the use of the guinea-pig was of any value. Even if the test gave a positive result, she would still think it wrong, as a matter of principle.

DR. SNOW, *November 28, 1906*

Herbert Snow, M.D., formerly Senior Surgeon to the Cancer Hospital, Brompton, gave evidence at the instance of the Parliamentary Association for the Abolition of Vivisection. He said that his evidence would be given to show "that the thing which has been reported as cancer in mice is a totally different thing from the cancer of human beings." He referred to a Report of the Imperial

¹ This Society was responsible for the notorious leaflet which was circulated a few days after the death of H.M. the late King, suggesting that His Majesty's death was due to medical treatment.

² Mrs. Cook subsequently wrote to the Commission, stating that she was unable to furnish the details referred to, without access to the Home Office files; and requesting to be heard again.

Cancer Research Fund, and stated that an article in it was to his mind "rather disingenuous." He referred also to an account, in the same Report, of a tumour of dogs, which in 1889-90 was regarded by competent pathologists as cancer, but was now set down as inflammatory; and to the early opinions as to actinomycosis and coccidial disease. He agreed with the general view, that the main feature of cancer is the multiplication of cells by division, and the formation of secondary growths by the distribution of these cells to other parts of the body. He said that he was afraid that the medical profession did not consider him as an authority on cancer, and had mostly ignored his work; he was not connected with a medical school. He had not himself investigated any mouse tumours, and only knew of them by literature. He had never seen any mouse tumours, either in the recent state or under the microscope. He was not aware of the fact that specimens of tumours, forwarded to the Imperial Cancer Research Fund from all parts of the world, were examined, and that a report was written on nearly every one of them and returned to the sender. He would not allow that the workers at the Imperial Cancer Research Fund Laboratory were familiar with human cancer. He had suggested to the Cancer Research Committee that they should call a congress on cancer. He had taken no part in the recent International Congress on cancer. He had not been doing any cancer work for the last year or two; he was getting old. It was perfectly unheard of, that cancer should disappear in the human subject. As a student he had never seen any experiments on animals. He would not say that it was not necessary sometimes to make experiments on animals, either painless or painful; but experiments should only be exceptionally allowed. He would not make any distinction between one animal and another. He thought

that experiments on animals should be allowed in special cases, with perhaps greater restrictions than at the present moment. He had no personal knowledge of cancer in the lower animals. Asked for his opinion as to the rarity of cancer appearing, in animals, at the seat of injury, he said that he did not know. Asked his opinion as to the theory that cancer is of parasitic origin, he said that there were *a priori* grounds against a genuine parasite in cancer, and that none had been found. He wished to withdraw the word "disingenuous," which he had applied to the paper published by the Imperial Cancer Research Fund. With regard to Jensen's tumour, he held that Professor Ehrlich and Professor Sims Woodhead had made a blunder. He thought that the clinical features of cancer were infinitely more reliable for diagnosis than the microscopic examinations of the growth by an expert authority. He said that he was afraid he was getting very rusty now, and for some years had not followed closely the general lines of research. He thought it justifiable to try a new drug on an animal. He would like to see the Act abrogated with reservations. He had never read the Act. He explained his opinion as to the objects of the use of curare, but said that he had never had anything to do with curare, and that his knowledge was rather rusty. He was asked his opinion of the statement as to keeping animals under complete anæsthesia for two or three hours, and said that he had no practical knowledge with regard to that. He was of opinion that cancer research had led to entirely negative results, both in this country, on the continent, and in America. He would recommend the granting of a special licence to any one who had any special line of research to pursue practically, for cancer, as for anything else, if he had any special points to investigate.

MISS ARABELLA KENEALY, *February 27 and
March 13, 1907*

Miss Arabella Kenealy, L.R.C.P. (Dublin), gave evidence representing the Parliamentary Association for the Abolition of Vivisection. She said that she had practised for eight or nine years, had ceased to practise seven or eight years ago, and since that time had been wholly engaged in literature. She made a preliminary statement condemning experiments in physiology, with special reference to the facts of internal secretion. "The term 'internal secretion' is one," she said, "upon which physiologists fall back when, after innumerable experiments on animals, they have failed to obtain any knowledge of the uses and functions of the organs investigated." Again, "the truth is, that the term 'internal secretion' stands for that higher and intrinsic function performed by all organs—a function which is beyond the reach of experimental physiology, and which is, I think it cannot be doubted, to be found in the realm of psychology."

Asked to give some particular instances of experiments in physiology which seemed to her useless, she referred to some experiments published in *The Journal of Physiology*, August 1906, and elsewhere. "I think, very likely," she said—"this is only my personal opinion—but, if one knows anything of the phenomena of hypnotism, one cannot help thinking that very often these animals must be entirely under the hypnotic influence of the man who operates. Consequently he gets what he expects to get; and another man, seeking for something entirely different in the same way, gets entirely different results." Asked about the production of immunity, by means of inoculation, she said that she considered it unscientific as well as futile. She said that if you made people immune to the microbe, you

made them insensitive to the deleterious conditions which breed the microbe; by the inoculation of a serum, you merely conferred a tolerance to the circulation in the blood of disease-germs which the healthy body should rebel against. She was of opinion that experiments, even if painless, were immoral, and consequently had not led to any beneficial result. She was asked the grounds for a statement in her open letter, that "the present great object of medical science is to discover in the blood of one or another of the lower animals 'cures' for all ills, or semi-miraculous substances which, injected into the blood, shall render us 'immune' to diseases we have incurred by neglecting natural laws." She said that she did not think that statement was a bit too strong. Asked about the micro-organisms of tubercle, typhoid, and glanders, she said that our knowledge at present was only sufficient to show that they were incidents in the disease, but that we had not knowledge sufficient to show that they were the cause in any disease. Asked to explain immunity, she said that all the immunity we can confer is by accustoming people to noxious elements.

Asked about diphtheria antitoxin, she replied, "I think that antitoxin treatment is what is known in medicine as an abortive treatment, like, for example, the abortive treatment of gout by cold applications; or, as when a person is suffering from catarrh, the use of medicated snuff—that is an abortive treatment. It stops the catarrhal process in the nasal mucous membrane, and drives it on to the stomach and liver. It violently changes the normal course of disease." Asked how she could explain why the use of antitoxin, in tracheotomy cases, had reduced the mortality by more than 20 per cent., she said, "I think that statistics are so deceptive that one cannot trust to them." Asked further as to antitoxin, she said: "My

position is, that in all diseases Nature selects the organ that shall throw out the poison. In diphtheria Nature has selected the tonsils as the best route for eliminating the poison. By giving antitoxin, you may relieve the tonsils, but you throw the poison back into the blood, so that it causes paralysis." She would make the use of the serum treatment a legal offence. She had no practical experience of any form of serum treatment. She had been out of practice for some years, but she thought that the onlooker saw more sanely.

She was asked whether she had had any experience of serum treatment, either curative or protective, in the case of animals; and she answered, "No, of course I have had no experience at all in the treatment of animals." She was asked whether she had ever used adrenalin, or seen it used, and she answered: "No, but I should find it quite unnecessary. We have quite enough beautiful healing medical substances from the mineral and vegetable world without going to the bodies of the lower creatures." She was asked whether the true interpretation of the different sounds in the heart had not been proved by experiments on animals, and she answered, "I think if they had they were unnecessary." She was asked as to the use of vaccines obtained from the patient himself, and she answered, "I think that when hypodermic injections are given, it is quite impossible to calculate the result; that there is no doubt of the hypnotic influence of the touch of the doctor; and, although it is not generally recognised, I believe that there are healing forces in certain people, and that very often these produce effects which we attribute to drugs which have been given hypodermically." She would disapprove of experiments for the diagnosis of disease. She would disapprove of experiments to keep plague out of the country, or to guard the public

health in any way. She was afraid that there were but few observers of other phenomena than those of serum treatment; but she did not say that medical science was absolutely unintelligent. She was asked about the infectious diseases of children, and said that the children certainly ought to be guarded against them by isolation; she was not sure whether it was for the advantage of children that they should have these diseases, but, as they always occurred, she could not help thinking that they might have some developmental effect, or that they were merely tests to weed out roughly those who were not among the fittest for survival. She had never seen any experiment on any living animal. Asked about digitalis, she said that she would not value the evidence of its effects upon a dog a bit. Asked about her Society, she said that there were only two doctors in it, Dr. Helen Bouchier and herself.

Asked about the contagious diseases of animals, she said that she knew nothing about animals. Asked about tetanus among horses, she said: "Nature has not for mischief, nor for malice, sown the earth with tetanus germs. The earth has been sown, if it is sown, with tetanus germs, in order to test the healthy or diseased state of these cattle." Asked about rinderpest, she said it was probable that when an animal had recovered from rinderpest it was scarcely possible to kill it with rinderpest afterwards; but she thought it absurd to suppose that the protective qualities of its blood could be transferred to another animal; and she would seek to restrain farmers from using these remedies. Asked about vaccination, she said that calves were very susceptible to tuberculosis, and we could not know that we had not conferred upon the human infant, in the tender infant years, the great susceptibility of calves to tuberculosis. She did not think

it possible for dogs to be absolutely anæsthetised during long experiments. She thought that curare was given to a very great extent. Asked about science and practice, she said: "The ordinary medical practitioner is not a man who makes discoveries in science. He is not a man of practical value to medical science. He is for the most part a rule-of-thumb person, who believes what medical scientists tell him." Asked about her former statement concerning the hypnotism of animals, she maintained that the operator, in the course of an experiment on the brain of any animal, might unconsciously suggest, to the animal's sub-consciousness, that certain muscles should move in response to the stimulation of a certain area. Asked about vaccination, she called it a disgusting and dangerous practice, and said that it may have increased the tendency to cancer. Asked about the discovery of adrenalin, she said that she did not consider it at all valuable. Asked as to the facts of experimentation on the motor area of the brain, she said that they had not been proved. Asked about Mrs. Cook's evidence, she said that she had nothing whatsoever to do with her evidence.

MR. J. W. GRAHAM, M.A., *Wednesday, March 6, 1907*

Mr. John W. Graham, Principal of Dalton Hall, Victoria University, Manchester, appeared for three Anti-vivisection Societies. He referred first of all to certain recorded experiments, some in America, and some in this country, which, in his opinion, had caused acute suffering. He referred also to a letter from Dr. Borel in *The Pall Mall Gazette*, August 1889. He then read a statement of his views as to the ethical questions relating to experiments on animals. Asked whether morphia was an anæsthetic, he said that he had no knowledge on that point. He would

be willing that we should all do without the benefits, real or supposed, that have come from diphtheria antitoxin. Asked a similar question as to the use of the preventive treatment against rabies, he said that he differed from Sir William Church's premises, and therefore declined to answer any question of the kind. Asked whether he thought experiments under anæsthetics immoral, he answered: "If you could guarantee that there was complete anæsthesia and not pain I would have no objection to the experiments; but, of course, the difficulty is to guarantee that. My objection is to the cruelty. If cruelty can be avoided—certainly avoided, and surely avoided—I am satisfied; then I should rejoice in the experiments." Asked about Dr. Borel, he said that all he knew about him was from the letter in *The Pall Mall Gazette*. Asked whether he saw any objection to allowing an animal to recover from an operation under anæsthetics, provided there was no serious pain afterwards, he said that he did not.

He was asked the following question, "Then may I put it in this way—that the opposition is on this ground: that, while you feel that it is impossible to go back upon what has been done, you do object generally to the animal creation being exploited painfully for the discovery of either prophylactics or cures of diseases in human beings?" and he answered, "Yes, when it is attended with great pain."

MISS L. LIND-AF-HAGEBY, *May 1 and June 5, 1907*

Miss Lind-af-Hageby stated that she had taken an active part in the agitation for seven years. Her interest had been roused by a visit to the Pasteur Institute, in Paris. She and Miss Schartau were joint authors of a book called "Shambles of Science." She read a short statement on the

ethical side of the question. She gave an instance, showing that, in her opinion, experiments were demonstrated at lectures which were not absolutely necessary for the due instruction of students. She also gave a list of text-books of physiology which contained no special warnings against the infliction of pain, or special instructions as to the duty of taking care of animals. She quoted passages from text-books advising, for this or that occasion, that an animal should be lightly, or not too deeply, anæsthetised. Asked, with regard to one kind of these experiments, whether she did not know that, if there was any pain, the experiment could not be carried out at all, she answered, "I do not think that is so."

She said that she assumed that the English vivisector, when he could get a better result with pain, defied the law. She also referred to Dr. Hill's book, "*The Physiology and Pathology of the Cerebral Circulation*," which, she said, contained, "clear indications that chloroform and ether are distasteful to the vivisector." Morphia, she said, was not an anæsthetic. She was asked, "In the case of a dose which is perfectly fatal, when the animal is not excited, but when it has become perfectly unconscious, do you think that animal feels pain?" She answered as follows: "My answer is that we do not know, and that the evidence is overwhelming on the side that that animal may be feeling pain, although it may be lying perfectly still." She proceeded to give references to certain text-books of physiology, which she said showed callousness. She then referred to Sir Lauder Brunton's book, "*Collected Papers on Circulation and Respiration*," as a conspicuous example of demoralising literature.

She went on to say that she had been present at several experiments at which the anæsthesia was not complete or satisfactory from a humane point of view. She said

that she objected on principle to all experiments on living animals, whether anæsthetised or not, and she was prepared to say that nothing which she would call useful had been obtained by them.

The witness was also examined on the case of the "brown dog." She declared that the Lord Chief Justice had said, in the course of the trial *Bayliss v. Coleridge*, referring to "The Shambles of Science," "This book is not libellous."

She did not believe that tuberculin was valuable as a test for tuberculosis in cattle. Asked about the immunising of cattle in South Africa against cattle-plague, she said that she had no evidence that they were immunised. Asked about vaccination, she said that she objected to it for the sake both of the infant and of the calf. She objected absolutely to the use of diphtheria antitoxin. Asked about Major Rogers's experiments on snake venom, she said that it would be better if he had used himself. Asked about sport, she answered, "My personal opinion would be that the vivisector (though I am a very strong anti-vivisectionist) who honestly thinks he works for the good of humanity, or the good of science, and who honestly tries to render his experiments painless, is on a higher moral scale than the man who seeks his pleasure from maiming and hurting animals."

Asked whether she would object to a surgical experiment on an animal, if it were absolutely painless, and if the animal were killed immediately after, she said that she had already said that she would not object to it merely as an anti-vivisectionist, but would object to it from a general humanitarian point of view. Asked about her statement that the spinal cord of a certain marmot had been divided, whereas Mr. Thane stated that he had examined the marmot's body and found that the spinal cord had not

been divided, she begged the Commission to excuse her from accepting Mr. Thane's statement. Asked about a certain statement in "The Shambles of Science," she answered, "I can say that there are vivisectors at present licensed and holding certificates in England who, I think, would be quite capable of exercising the 'black art of producing the utmost agony, while the gateways through which death could enter are carefully watched.'" She was asked further, "Do you present this as a fair and true representation of vivisection as practised in England to-day, that animals have their 'pain-tortured bodies, trembling under the sharp steel, bathing in their own blood, and in vain trying to tear the straps that fasten them to their crosses of agony'?" She answered "Yes." She was examined further as to certain statements furnished to the Commission concerning "The Shambles of Science." She was also examined as to the meaning of the phrase "purposive movements," and as to the question of the painfulness of going without food, and as to further points in "The Shambles of Science," and about diphtheria antitoxin, and about the use of the term "pithing." Asked as to her statement that nothing useful had been discovered by experiments on animals, she said that she ought to have added "nothing useful that could not have been discovered by other means." Asked whether she considered the preventive treatment against rabies useful, she said that she did not. She did not consider adrenalin useful. Asked whether anything useful had been discovered for the treatment and prevention of anthrax, she said, "No, certainly not."

THE REV. JOHN PAGE HOPPS, *May 15, 1907*

Mr. John Page Hopps appeared on behalf of the Social Purity Alliance. He made a general statement; and was

then asked whether he was acquainted with the law relating to experiments on animals, and whether he thought people vivisected without a special object. He said that he knew nothing of his own knowledge as to experiments on living animals. He did not know of any practicable suggestion as to any alteration of the law in the direction of further restriction. He was told by one of the Commissioners, "I am afraid that you did not realise your responsibility." He was not a vegetarian. He did not know that there was any difference between one sort of beef and another. He shrank from eating meat at all. Asked whether it was wrong or right to kill the animal of which he last ate a portion, he said that he was "in doubt about it." Asked whether it was wrong to kill animals painlessly in order that their hides might be turned into leather, he answered, "That again is a doubtful question." Asked what made the difference between a baby and a sheep, he said, "The vivisector is bound to answer me that." Asked whether he would object to painless experiments on animals, he said, "I cannot imagine it; I do not trust my fellow creatures enough." Asked what was his test of what is ethically right or wrong, he answered, "My own conscience, my education, and my general spiritual condition." He was asked, "Have you not given evidence to-day on what you think is the case, on many points, without having made any inquiries as to whether it is so really?" And he answered, "I cannot say that; I do not admit that at all."

THE REV. L. S. LEWIS, *May 29, 1907*

Mr. Lewis represented the Church Anti-Vivisection League. He stated that he would not have one mouse painfully vivisected to save the greatest of human beings, nor the life dearest to himself. He thought it was not

wrong to kill for food. He would not disapprove of painless vivisection. He said that he was not only an anti-vivisectionist; that he attacked all cruelty. Asked if he had ever yet attacked the cruelty of mutilating many millions of animals every year by unsexing them, he said that it had not been brought to his mind, but he had thought about it for some time. He then was asked some questions about sport. He was afterward asked about diphtheria antitoxin, and said that some years ago he thought he was going to have diphtheria, and expressed his intention of not having antitoxin. The following question was then put to him:

Q. "Now I want to put this question to you. I suppose most of us have seen children die from diphtheria—helpless children. There is not great courage in letting a helpless child die from diphtheria. Do you think you are justified, to save the life of a helpless child, in using antitoxin or not?"

A. "I would not allow my own child to have antitoxin administered to it to save its life. You should not do evil that good may come. The thing is wrong in itself; it would not exist in the world if it had not been for vivisection: it is not the sort of thing that might have been discovered by something else."

Asked whether he would let the child die, he said, "I should." He was an anti-vaccinator. He said that a new remedy for snake-bite ought to be tried on men. Asked about the preventive treatment against rinderpest, he answered that he did not think that we had the slightest right to make those experiments. He went on to speak of certain experiments which had been made on hospital patients.

MISS A. L. WOODWARD, *May 29, 1907*

Miss Woodward, Acting Secretary of the Church Anti-Vivisection League, stated that a medical man at Bradford, in 1898, not holding a licence under the Act, had publicly declared that he had "performed hundreds of vivisections." Asked whether these were not experiments in the preparation of lymph for vaccination, she said that she could not say what he meant. She gave instances of complaints against experiments, made to the Home Office, but without result. She also referred to a text-book on "Animal Physiology," 1892, used by pupil-teachers. She said that some of the Slade School female students, in 1903, had complained of the noises made by the animals kept at University College. She knew nothing about it herself. She said that she had lately interviewed a young man who at the age of fourteen had thrown up, for fear of becoming cruel, his situation in a laboratory. Referring to "The Shambles of Science," she said that she had refused to give up her copies of the first edition. "Every year," she said, "we have offered that first edition publicly for sale to the public at our stall at the Church Congress."

MR. G. H. BURFORD, M.B., *May 29, 1907*

Mr. Burford, Gynæcologist to the London Homœopathic Hospital, gave evidence at the request of the World League of Opponents of Vivisection. He was not a member of it. Asked whether it had 100, or 1,000, or 5,000 members, he answered, "It is perfectly immaterial to me." He gave instances (colchicum, belladonna) of the homœopathic method of studying drugs on volunteer experimenters. He mentioned certain remedies, previously unknown or ignored, which had been introduced into

professional use through homœopathic methods. Asked about Dr. Cushny's evidence as to digitalis, he said that the experiments described by Dr. Cushny did not add anything to our knowledge of the remedial virtues of digitalis. He had not seen any vivisections in England since he was a student, before the present Act. He admitted that useful knowledge had been obtained by experimentation on animals. He agreed most emphatically that serum therapy was of use. He did not contest the proposition that useful drugs had been introduced into therapeutics as the direct results of experiments on animals. He agreed that veterinary drugs ought to be studied by experiments on animals. He frankly admitted that the protective sera had been discovered entirely by animal experimentation; but he thought that they might have been discovered by the methods of homœopathy. Asked to suggest any amendment of the Act of 1876, he suggested that experiments on the remedial action of drugs should be carried out on healthy persons, volunteering for that purpose. Asked about vaccination, he said that in the majority of instances he admitted the utility of vaccination. He described a homœopathic method of administering vaccine lymph by the mouth. Asked about physiology, he said that nobody could deny that physiological knowledge had been advanced by experiments on animals. Asked about surgery, he expressed his decided conviction that it would be better to try a new operation in surgery first on a human being and not on an animal. He pointed out that Mr. Lawson Tait "publicly foreswore any alliance or allegiance that he had to knowledge vivisectionally obtained."¹

¹ It is true that Mr. Lawson Tait mostly denounced all experiments on animals. But, in 1893, a meeting was held, in Birmingham, of medical men, to promote the formation of the British Institute

THE HON. STEPHEN COLERIDGE, *June 19 and 26, July 3,
and December 11, 1907*

Mr. Coleridge, representing the National Anti-Vivisection Society, stated that his Society was far the largest in the world, and had received, for the last five years, £36,097. He had put down, so that there should be no question about the claim of his Society really to represent the anti-vivisectionists, the force and strength of the other Societies. He described to the Commissioners fourteen other anti-vivisection societies with incomes ranging from about a quarter of his Society's income down to £6 17s. 6d. The Birmingham Branch of his own Society was very large, but some of its members only subscribed a penny or twopence or sixpence. He spoke of what his Society had accomplished, saying that it had prevented a public meeting at the Mansion House in 1889, with the result that no Pasteur Institute was set up in England. Another remarkable achievement of his Society, he said, had been the long agitation against the "diversion" of Hospital Funds, in London, to Medical Schools. "We claim," he said, "to represent the consensus of opinion of almost all the greatest names that have adorned the history of England in the field of thought." He then gave a list of twelve charges against the Home Office. These charges were as follows:

(1) I am here to charge the Home Office officials with repudiating the most important duty deputed to them by

of Preventive Medicine, which is now the Lister Institute. At this meeting, Mr. Tait supported the resolution approving the objects of the Institute. He said that he fully assented to the resolution, feeling that, while he objected to a certain class of surgical investigations, bacteriological experiments on animals had proved of great value. (See Sir Victor Horsley's evidence before the Commission, Chapter V.)

Parliament, *videlicet*, that of protecting animals from unjustifiable suffering.

(2) I am here to charge the Home Office officials with having constituted themselves the injudicial defenders of the vivisectors from criticism by my Society in the past, and in their evidence tendered before this Commission.

(3) I am here to charge the Home Office officials with having appointed inspectors who have displayed such bias that they have thought it their duty not to make detective efforts to protect animals from illegal treatment.

(4) I am here to charge the Home Office officials with having made entirely disingenuous statements in their official utterances, and with having constituted themselves the mere spokesmen of the vivisectors.

(5) I am here to charge the Home Office officials with accepting the suggestion made to them by some nameless adviser that to starve animals for days is not cruel, when they must know perfectly well that if any one of them starved their own horses (if they have any) the nearest policeman could successfully prosecute them for cruelty to animals under Martin's Act.

(6) I am here to charge the Home Office officials with suppressing in the Annual Parliamentary Return the names of those who take upon themselves the very grave responsibility of signing the certificates exempting licensees wholly or in part from the obligation to employ anaesthesia in their vivisections, although in the Report of the former Royal Commission we find this clear direction: "We recommend that his (the Home Secretary's) advisers be from time to time elected and nominated by himself. Their names should be made known to the profession and the public."

(7) I am here to charge the Home Office officials with shielding the names of such licensees as they know to have

broken the law, although the former Report contains these words: "Abuse of the power conferred by the licence must, of course, render the holder liable to its withdrawal, but this will involve great disgrace," a phrase that clearly indicates that the framers of that Report contemplated the publication of offenders' names, for no great disgrace can appertain to a secret remonstrance or a secret withdrawal of a licence.

(8) I am here to charge the Home Office officials with preparing for the Home Secretary evasive and insufficient replies in the House of Commons to plain questions on the administration of the Act, and with making evasive and insufficient replies themselves in official correspondence with my Society, and with leaving perfectly proper questions unanswered altogether.

(9) I am here to charge the Home Office officials with putting forward annually a Parliamentary Return in which it is asserted, on the official authority of the Government Department, that not a single experiment in thousands inflicted upon animals in Great Britain with its permission can be specified as entailing any pain at all, when all the while this plausible assertion is based upon no better evidence than the bare assertion of the vivisectors themselves, who are not to be expected to report themselves cruel men.

(10) I am here to charge the Home Office officials with placing a certain vivisector year after year beyond the reach of the safeguards erected by the Act to protect animals from illegal treatment by licensees, by giving him permission to vivisect in private places, thereby placing him beyond the possibility of legal inspection; because by Clause 10 of the Act of 1876 the inspector has no right of entry into, or inspection of, any unregistered place.

(11) I am here to charge the inspectors certainly, and the Home Office officials apparently, with having made no

inquiries, public or private, into the vital question of the character for humanity of the licensees to whom they have delivered over the animals to be vivisected. Although those officials must be aware that the Report of the Royal Commissioners in 1876 contained this pregnant warning: "It is not to be doubted that inhumanity may be found in persons of very high position as physiologists," and although the Act of 1876 placed upon the Home Secretary the tremendous responsibility of personally protecting animals from that undoubted inhumanity.

(12) I am here to charge the Home Office officials with having placed themselves in improper private confidential relations with a private society composed of supporters of vivisection entitled to no more consideration than the National Society composed of opponents of vivisection.

Over charges 1, 2, and 3, Mr. Coleridge referred to the "brown dog" case, to certain experiments on resuscitation after drowning, to certain experiments made by Dr. Crile in this country,¹ to the meaning of the phrase "incomplete

¹ It was pointed out to Mr. Coleridge, that Dr. Crile, in a letter to the Inspector under the Act, had stated emphatically that in no instance did any of the animals used in these experiments suffer pain. It was also pointed out to him that Sir Victor Horsley had witnessed a good many of these experiments and was of opinion that no pain had been caused to the animal. It was also pointed out to him that Dr. Goodbody, also, had witnessed these experiments. Mr. Coleridge answered that he would not accept Dr. Crile's statement at all, and that he would certainly not accept Sir Victor Horsley's statement. "I think," he said, "that all these experimenters have the greatest contempt for the Act of Parliament. They would deny a breach of this Act, just as I should deny a breach of the Motor Car Act. I drive a motor car, and when I go beyond the speed limit, and a policeman asks me, I say, 'No, I am not going beyond the speed limit.' Nothing would keep me from going beyond the speed limit, except the presence of a policeman in the car; and nothing will keep an experimenter within the four corners of the Act except an Inspector in the laboratory."

anaesthesia," and to the wording of Certificate B. He also referred to a demonstration of snake venom on three rabbits; to the Harveian oration given by the late Dr. Poore in 1899; and to the difficulty of finding out how certificates under the Act are signed, and how long they are effective. Over charge 4, he referred to certain statements made by Government officials; to the case of the "brown dog"; and to some experiments reported in *The British Medical Journal*, June 18, 1898. Asked whether he was suggesting that the two gentlemen concerned in those experiments had said what was not true, he answered, "That is my suggestion." Over charge 5, he referred to some experiments published 1895-6. Over charge 6, he alluded to a correspondence between himself and the Home Office in 1899 as to the signature of certain certificates given to Professor Schäfer. Over charge 7, he said that he had tried in vain to get from the Home Office the information that he desired. Over charge 8, he said that the Home Office in 1899 had given certain answers which were wilfully evasive. He also referred to certain answers in 1904. Over charge 9, he referred to a form of the Parliamentary Return which has now been abandoned. Over charge 10, he stated that he had sent a protest to the Home Office in 1905, and that no notice whatever had been taken of it. Over charge 11, he referred to certain evidence given before the first Royal Commission in 1875. Over charge 12, he referred to the constitution of the Association for the Advancement of Medicine by Research.

Mr. Coleridge then spoke of certain amendments of certain suggestions embodied in his Bill for the amendment of the present Act. He was asked certain questions in ethics. "If an animal," he said, "be placed under complete anaesthesia, and destroyed before it recovers consciousness,

personally I have no objection to that vivisection at all; and anything that might be discovered thereby would be to the benefit of humanity and welcomed by myself." And again, "If the pain be really and truly eliminated from vivisection, I personally have no objection to it." And again, "With regard to the results of vivisection, I see no objection to human beings benefiting by the results, provided that those results are not continually produced by fresh infliction of pain, and serious pain." Later, he was asked the following question, "If any suffering is caused, you would stop even what skilled people say is a most useful investigation?" And he answered: "I am not an unreasonable man. When I say suffering, I mean real severe suffering." Asked whether discomfort incidental to certain diseases would come within that definition, he answered, "If the discomfort amounts to what is really serious, so that the animal is utterly wretched, I say that the animal ought to be killed." And again, "Where the suffering is really trifling, of course, I do not want to interfere."¹ Asked about Certificate B, which permits the keeping alive of an animal after an experiment performed under an anæsthetic, he said that Certificate B would be abolished by his Bill. Later, he said that he would only agree to Certificate B being continued provided that the animal after the operation was as much safeguarded as it would be under inoculation.

On December 11, 1907, Mr. Coleridge was recalled and further examined as to certain opinions expressed concerning his evidence by other witnesses.

¹ On July 3, 1907, Mr. Coleridge said that he would be willing to insert "serious pain" in his Bill. Later, he said in a letter to the Commission that he felt it would be unwise to qualify the word "pain" with any adjectives in his Bill. See his evidence, December 11, 1907.

MR. STEPHEN SMITH, *October 29, 1907*

Mr. Stephen Francis Smith, M.R.C.S., stated that he was surgeon to the Anti-Vivisection Hospital. Asked whether he thought that the sense of pain in a West African negro was greater than the sense of pain in a cultivated white man in England, he answered, "I should think probably it is greater." Asked about the use of curare on an animal under an anæsthetic, he said that the anæsthetist could not tell, by the eye, or breathing, or anything else, whether the animal was fully under the anæsthetic. He proceeded to speak of experiments on the action of drugs, and made the following statement, "The evidence is overwhelming that, whether with old drugs or new, any conclusions from experiments on animals are absolutely untrustworthy, and would be dangerous if relied on." Referring to the discovery that mosquitoes convey malaria, he said that experiments on animals had nothing to do with it. Referring to myxœdema, he said that the cause of this disease was thoroughly and completely understood before any experiments were made on animals. Asked about rabies he said, "The pretensions of Pasteur's treatment rest upon statistics, and by statistics they are condemned."¹

¹ The witness referred to the cases of Arthur Stoboi and Henri Moens, and said that the dogs which bit them remained healthy. But, in the case of Moens, as a matter of fact, the dog was pronounced rabid by M. Dell, a veterinary surgeon, who saw it during life and dissected it after death. The dog that bit Arthur Stoboi ran off and could not be traced. Mr. Smith said, of Arthur Stoboi, that "the disease was preceded by pains at the seat of inoculation." It is not probable that Stoboi ever had rabies: he died suddenly; he was merely feverish for two days, with headache and slight restlessness, but nothing alarming in his general condition. He complained of pain, not at the site of inoculation, but in the neighbourhood of the bite. (See the *Annals of the Pasteur Institute*.)

He then spoke of diphtheria, and stated that Dr. C. J. Martin's evidence was "simply honeycombed with fallacies." He was then examined as to a book that he had written. He stated that he had done no experiments on living animals himself, and had never worked in any English laboratory. He was in general practice from 1884 to 1899. He was not conversant with English laboratories. He was asked to explain statements in his book, that "curare is used daily throughout England," that "morphia is not an anæsthetic," etc. Asked about diphtheria, he said that he did not know how antitoxin is standardised; and that he was unable to explain the fact that an animal, poisoned with diphtheria toxin, takes no harm if it receives the antitoxin in a sufficient quantity. He had had no experience of diphtheria since the introduction of the antitoxin. He had never had to do tracheotomy.

Asked whether his opinion, that it was difficult to keep wounds on animals surgically clean, was justified by his own experience, he said that it was only as regarded his own dog. Asked about the diseases of animals, he answered that he knew nothing about them. Asked whether he was aware that the veterinary pharmacopœia was practically the same as the human, except with regard to doses, he answered that he was not. He was then examined further as to typhoid fever and rabies and diphtheria. He was asked about his hospital, and said that they had used no antitoxins or sera, and would not take in cases of diphtheria, if it could be helped. Asked why, he said that they had only one isolation ward: he did not think it was always furnished, and it was in a very inconvenient place. Asked whether he could bring before the Commission any single fact on which he could base his belief that cutting operations on animals not anæsthetised take place in England, he answered, "No, not a single fact." Asked whether he was

aware that, if he had brought the diphtheria figures in his pamphlet down to 1905, it would have shown a decreasing mortality since the use of serum became more common, he answered that he was not aware of that fact.

MR. GEORGE GRANVILLE BANTOCK, M.D., F.R.C.S. Ed.,
November 6, 1907

Dr. Bantock stated that he had been asked to give evidence by the Chairman of the Parliamentary Anti-Vivisection Association. He did not belong to any anti-vivisection society. He said, "The bacillus is never the cause of the disease; it results from the disease; the conditions are favourable for its development: for we are swarming with bacilli of various kinds, and it only wants certain conditions to favour the development of certain bacilli." He made a statement of the grounds of this belief. He was asked about the sterilising of water by boiling, to wash or irrigate wounds. He answered that he never boiled the water. Asked whether Mr. Lawson Tait did not boil the water, he answered, "I do not think he did; he did not boil all the water, anyhow." He said that at the Samaritan Hospital he had a record of ninety successive successful ovariectomies without the use of any antiseptic whatever. In his own practice he made absolutely no use of sterilised dressings. Asked whether he was aware that filtered vaccine fluid does not convey the disease, he said that he was not aware of any observations in that direction. He said that it was utterly incomprehensible, to his mind, that smallpox should spread through the action of a special form of bacillus. He was also asked questions about tuberculosis, and about diphtheria. He was asked to give his authority for stating that the bacillus of tuberculosis was favoured in its growth by air, light, and

sunshine. He said that this was his own view, and that he was not aware of anything contradicting it. Asked as to his statement, that not a single case had ever been put on record in which *tabes mesenterica* had been communicated by milk, he said that he affirmed that most confidently of the human subject; he put absolutely out of account all experiments on animals. Asked about his statement, "Ridicule seems to be the appropriate weapon to use against the theory that plague is conveyed to the human subject by rats through the agency of fleas," he said that he thought so. Asked about the tse-tse fly, he said that it developed its own poison, like a wasp or a bee. Asked about tick fever and sleeping sickness, he said that he did not know anything about them.

Dr. Bantock said that he had never seen an experiment on an animal. Asked about the relation between germs and diseases, he answered that he thought that the micro-organism was the result of the conditions, "just as you find grouse growing on moors, and partridges and pheasants on arable land, and salmon in running rivers, and so on." Asked his opinion of the preventive treatment of rabies, he said that Pasteur's followers claimed that "every case, in which the disease is not manifested after inoculation, is a case to be regarded as a cure." He was asked whether he had any knowledge whatever about diseases of animals, and he answered, "No, I do not profess to know." Asked as to his statement that "dirt" was a thing to be avoided in wounds, he answered that "dirt," apart from germs, might poison a wound. Asked to explain what he meant by "dirt," he instanced a man cutting his head on the road, and getting sand into the wound. Later, he was asked what it was against which he guarded when he was "cleanly"; and he answered, "Dirt; and that is the only explanation I can give you." Asked what it was, in dirt, that

he desired to guard against, he answered, "I do not know, nor does anybody know." Asked about anthrax, he said, "I know nothing at all about it; I have not studied the subject." Asked about diphtheria, he said that he had not had any experience with the antitoxin. Asked about the justification of the use of animals to prepare vaccine, he answered: "I have no sentiment in the matter of experiments on animals. With me it is a question of utility, and if I can be shown that certain experiments are valuable in increasing our knowledge, I have no objection to them." Asked whether he had read the two Reports of the Plague Commission, he said that he had not. Asked further about the justification of the experiments on animals, he said: "I would simply go to the limitation of experiments. I am not an opponent of vivisection. I might say that I should be almost sorry if vivisection were abolished, because it may be useful yet. It has yet to be proved that it has been of material service, but it may yet be so. The only opposition that I should offer to vivisection would be that operations should not be performed before a class, that experiments of which the results are already known ought not to be repeated, and that experiments with a view of ascertaining the action of remedies ought to be very much curtailed. That is the extent to which I should go." Asked about a Pasteur filter, he answered, "I am not aware that there is a filter which will prevent microscopical objects from passing through."

LIEUT.-COLONEL LAWRIE, I.M.S., *November 20, 1907, and
March 25, 1908*

Edward Lawrie, M.B. Edin., M.R.C.S., retired from the Indian Medical Service, gave evidence on anæsthetics in general and on the Hyderabad Commission. He also said

that when he saw Haffkine's vaccine used "it was always putrid." He was not asked any further questions on that point. He put in a printed statement, giving his account of an experiment made at Cambridge thirteen years previously, on July 7, 1894. The experiment was a cross-circulation experiment, made on two dogs. It was made in Dr. Gaskell's laboratory.

He saw the two dogs on the table, with tubes in their throats. He said that Dr. Gaskell told him that the dogs had had no anæsthetic except morphine. He also said that Dr. Gaskell led him to understand that the dogs had had the morphine so that Dr. Gaskell might be able to report that the dogs had had an anæsthetic, so as to hoodwink the inspector. Further examined, he said that Dr. Gaskell said that the dogs had had no anæsthetic at all except a small dose of morphine, ten drops of morphine solution. Further examined, he would not undertake to say that Dr. Gaskell had used the words "hoodwink the inspector." He said that he saw the animals again after luncheon; that they were not anæsthetised; that they appeared to be in pain; that they were shivering. "I thought at the time," he said, "it was excessively cruel—the cruellest thing I had ever seen." On July 11 he saw a similar experiment. He said that at no time while he was there did he see any chloroform administered except the concentrated dose that was given at the end of the experiment.

Having heard Lieut.-Colonel Lawrie, the Commission heard Dr. Shore, Mr. Hall, Dr. Anderson, and Dr. Gaskell.

Dr. Shore, M.D., Fellow of St. John's College, Cambridge, and University Lecturer in Physiology, had already been examined as to Lieut.-Colonel Lawrie's statements, and was now recalled and further examined. He said the conditions used for anæsthetising the animals were precisely the same on July 11 as on July 7, and were carried out in

precisely the same way ; and there was no difference, on the two days, between the conditions of the animals. Two dogs were used in each experiment. Before anything was done to the dogs one of them received ten cubic centimetres of a 2-per-cent. solution of morphia, and the other received six cubic centimetres. Thus the dose was thirty-six times stronger than appeared from Colonel Lawrie's statement. Then, before the skin was cut, chloroform was given to the animals. They were kept in a state of absolute anæsthesia before the cutting began and up to the time of death. He prepared the dogs himself. The morphia was given with a hypodermic syringe ; and the dogs were left for half an hour, till they became drowsy and laid down. Chloroform was then put to their noses, till they were unconscious and absolutely insensible. They were then lifted on to the table and laid on their backs ; they were secured just enough to keep them in the required position for the operation, and to prevent their rolling over, but not enough to prevent a good deal of movement, if the dogs had been conscious. A tube was then put into the trachea, and was connected with a bottle containing the anæsthetic mixture, before any further cutting operation was done. The dogs were kept under the anæsthetic all the time ; there was no struggling, nor any sign of pain. Dr. Shore was absolutely certain that Dr. Gaskell did not say anything about a ten-drop dose, nor anything about hoodwinking the inspector. He and others were away, for luncheon, about a quarter of an hour. While they were at luncheon the laboratory attendant never left the room, having had instructions to see that the animals were kept quiet, and, if necessary, give them a further A.C.E. mixture. When Dr. Shore came back the dogs were still perfectly quiet, and no A.C.E. mixture had been necessary. They were quite unconscious, and there was absolutely no

movement. The experiment was completed, and the dogs were killed by chloroform, both of them; they never regained consciousness, and they suffered no pain. He had no hesitation in saying that the whole of Colonel Lawrie's story, as to Dr. Gaskell stating that he had given the morphine only to hoodwink the inspector, was a fabrication from beginning to end; but he admitted that they might have left the room and carried on a conversation which he did not hear. But it was quite impossible that Dr. Gaskell could ever have made such a statement.

Mr. William Hall said that he was assisting in the laboratory on July 7 and 11. The operations were done in exactly the same way, as regards preparing the dogs, on each occasion. The morphia and the chloroform were given as Dr. Shore had stated, and the animals were put in position as he had stated. The dog was kept under chloroform all the time, till the tube was put in. He never left the room during the whole time from the giving of the morphia till the dogs were dead. The dogs were under his eye the whole time. He did not see anything of consciousness, nor any struggling.

Dr. Anderson, M.D., F.R.S., Fellow of Gonville and Caius College, Lecturer in Physiology in the University, said that he saw the experiment on July 11. He watched the experiment closely; he was quite convinced that the dogs were totally unconscious of pain. It was quite a misrepresentation to say that the dogs were tied tightly. They could have moved if they had been suffering. He was quite satisfied that there was no movement and no restlessness. He was asked, "If there had been any struggling, would it have interfered with the observation?" He answered, "As an experimenter myself, I cannot imagine the experiment being performed in that case." Asked whether the operation would be possible under

curare, he answered: "No, because the whole point of the experiment depended on watching the respiration. Curare would have neutralised the effect that we desired to secure."

Dr. Gaskell, M.D., F.R.S., Fellow of Trinity Hall, Lecturer on Physiology in the University, and a Member of the Royal Commission, was asked as to the dose of morphia which was given to begin with. He answered: "The actual dose is on the record written in my own handwriting; it was written on that very day. I looked it up the other day, in Dr. Shore's rooms; it was covered with dust, old and shabby, but there it was, the exact amount given. That was done, I know, on that very day. It was ten cubic centimetres of a 2 per cent. solution. That is what I wrote, and that is what I told Colonel Lawrie. If Colonel Lawrie manufactures it into ten drops of morphine, that has nothing to do with me." Asked whether he had said that the morphia had been given not to prevent pain, but in order to hoodwink the inspector, he answered: "I have told you I have not the faintest remembrance of what I did say, but I am absolutely certain that I never said that. I should never have dreamed of saying it, and it is such a silly thing to say." Asked whether Colonel Lawrie had said anything to him about the dogs being not sufficiently anaesthetised, or in pain, or anything of the kind, he answered: "I am absolutely certain that he never mentioned that; for that is a sort of remark that I should have remembered. When I came in—I cannot say whether it was just before Colonel Lawrie came in or not—the dogs, all the time I saw them, were absolutely quiescent. There was not a movement, or a sign of movement; and, in addition to that, the record shows that the blood-pressure tracing was perfectly steady, and the respiration was perfectly steady too. So that

those two things, which would have shown any indication of pain in the dog, as well as movement, were absolutely absent. I am as convinced as I am of anything in my life that those two dogs were fast asleep. I saw them during the whole time that Colonel Lawrie saw them. The whole time I saw them they were lying absolutely quiet. There was no movement, and they were breathing regularly and quietly."

MR. JOHN HUGHES, *November 26, 1907*

Mr. Hughes, Secretary of the National Canine Defence League, stated that his Society was founded, as an Anti-Muzzling League, in 1891. It objected to muzzling as cruel and ineffective. It was opposed to all experiments on dogs, either with or without anæsthetics. It had had a Bill before the House of Commons during the last three Sessions, but it had never been discussed in the House. It had also issued petitions, one medical and one general. The general petition made no reference to anæsthetics; it had received up to date 754,584 signatures.¹ The medical petition had received 1,261. He read extracts from some letters on the subject. He stated that at the last General Election they found that 380 members of the House of Commons were pledged to support his Bill. They had also sent a petition to the Home Secretary, and a petition to Her Majesty Queen Alexandra. His Society, he said, had "adopted the Buisson Bath as an antidote to hydro-

¹ Neither of these petitions made any mention of anæsthetics. The League also published a leaflet, headed "Sign the Nation's Petition to Parliament against the Dissection of Live Dogs in Medical Laboratories." This leaflet was so false, that it was denounced, by another anti-vivisection society, as a "deplorable leaflet, containing a series of grossly false and misleading statements." (See the *Times*, December 11, 1903.)

phobia.”¹ His Society had some medical men upon its Committee, or among its Vice-Presidents; but the personal opinions of these gentlemen as to the success of the Buisson Bath had not been obtained. The objects for which his Society was formed were “organised opposition to the muzzle, including the repression of dog scares, opposition to Pasteurism, and a Buisson Bath propaganda.”

Asked about other animals than dogs, he said that they could not go beyond the dogs; they were a Dog Society. His Society had 4,421 members; its income in 1906 was £1,698. Printing, that year, had cost £664; elections, lectures, dog shows, memorials, and travelling expenses, £133; advertisements, £116; office rent, £80; salaries and wages and extra clerical assistance, £487.

He was then examined as to his opinion of the inoculation-test for the diagnosis of rabies. Asked about the 672 cases of rabies returned by the Board of Agriculture in 1895, he thought it quite possible that they might none of them be rabies. Asked whether he could think of any circumstances in which the infliction of pain on an animal would be justified, he answered that he could not say that there might not be such cases, but he could not think of any at the moment. He was then asked how he justified a certain statement in his Society's petition to Parliament; and he said that he would look up a large number of cases in support of this statement, and would supply them to

¹ Dr. Buisson, under the impression that he had hydrophobia, took a vapour-bath, meaning to kill himself: “and at 42 degrees (127 Fahrenheit),” he says, “I was cured.” It was an ordinary case of imaginary hydrophobia. This vapour-bath is approved, by the National Canine Defence League, “on the simple common-sense principle that if poison is injected into a person's veins, the best thing to do is to get it out as quickly as possible.” The testimonials to this vapour-bath are such as might be expected. There is, or lately was, one of these baths at the Anti-vivisection Hospital.

the Commission.¹ He said that the petition was still being used. He was then asked about distemper, and said that his Society would totally disapprove of any inoculations or any experiments on healthy dogs, for the purpose of giving them distemper, even for the purpose of saving future generations of dogs from distemper. The same principle would apply to all canine diseases. Asked about the Dogs' Home, where about 30,000 dogs are killed every year, he answered, "We do not approve of that." Asked whether his Society was prepared to provide for 30,000 canine orphans every year, he answered, "I do not think we should find much difficulty in finding homes for them." Asked for the views of his Society as to the common operation on female dogs to prevent their breeding, he answered, "I do not think they have any."

MR. J. H. LEVY, *December 3, 1907*

Mr. Levy represented the Personal Rights Association, of which he is Hon. Secretary. This Association began in 1871. It had "quite an illustrious band of Vice-Presidents," and from 120 to 150 members. His evidence referred to ethics; he did not propose to give evidence in physiology. He was asked, "May I take it that you do not see any justification for allowing animals to come to, after an operation, in order to watch the results?" and he answered: "Provided that there were no pain connected with it of any serious kind, I should see no objection even to that. It is only a question of whether the animal is subjected to serious pain in the matter. I see no objection to an experi-

¹ These cases were not supplied by the witness, at the time of the evidence going to press. The statement was that, "on account of this docility and obedience to the word of command, the dog has of late years been specially selected by vivisectors for extensive and peculiarly revolting and painful experimentation."

ment on an animal as such." Referring to ethics, he discussed the Eugen Hahn case, Metchnikoff's Harben lecture, and Smidovich's book. He was then examined at some length on ethics. Asked where he drew the line between permissible pain and unpermissible pain, inflicted on animals not for their own good, he answered, "You cannot do it in practice." He was not a vegetarian. A man might be justified in killing animals simply because his comfort was threatened by them. The sensibility of the animal must of course be taken into consideration. Asked whether he would approve of legal prohibition of field sports, in so far as they were painful, he declined to answer, saying that he would not go into the matter. Later, he handed in Smidovich's book with the reference to Hahn, and the passage from Metchnikoff's lecture.

MR. CHARLES R. J. A. SWAN, M.B., *December 10, 1907*

Mr. Swan represented the National Canine Defence League. Asked whether he thought that experiments upon live animals properly conducted were essential to the medical profession, he answered, "Yes, they are." Asked whether he considered blood-pressure experiments on dogs, under anæsthetics, a desirable thing, he answered, "If it is done under anæsthetics, I do not think one ought to be inclined to veto it"; but he was not prepared to say that it could not be just as well done on a rabbit. We must have experimental knowledge; it enlarged our clinical observation, we knew where to look, and what to expect to find. He did not object to other animals being kept for observation after operation under anæsthesia, in cases where such observation was strictly necessary; but he would strain every nerve to leave the dog out. He was asked, "If experiments on dogs for scientific purposes were always

conducted under anæsthetics, would all your objection to using dogs be met?" He answered, "Yes." Asked whether he would exempt the monkey as well as the dog, he answered: "I have not got the same affection for the monkey. I only had one; it bit everybody; there was no affection about that animal." He admitted that a most valuable mode of treatment of kidney disease had been the outcome of experiments on animals. He had not the slightest doubt that a dog could be properly anæsthetised with chloroform. Asked about experiments on distemper, he agreed that the use of dogs for them was justifiable; it was in the interest of dog-land. Asked about the use of the gastric fistula in experiments on dogs, he said that all the processes of human digestion in the stomach could be thoroughly tested by syphoning the human stomach. He thought that the experimenters did as a matter of fact choose other animals rather than dogs if they could; and he was very glad of it. He was sure that they had the same feeling, with respect to dogs, as he had. That was his difficulty in giving evidence, because so many of them were his personal friends.

MR. R. J. COWEN, L.R.C.P., L.R.C.S. IRELAND,
December 10, 1907

Mr. Cowen represented the National Canine Defence League. He was not a member of that Society. He gave his reasons for thinking dogs so different from man that it was useless to examine them before the study of human diseases. He thought they were less suitable than rabbits or guinea-pigs. He thought it practically impossible to keep a dog fully anæsthetised for any length of time. He thought that in many cases of anæsthesia the pain was there, though the patients apparently had forgotten it when they awoke.

SIR GUILLUM SCOTT AND SIR FREDERICK BANBURY,
 BT., M.P., *December 11, 1907*

These two witnesses appeared together, as representing the Royal Society for the Prevention of Cruelty to Animals, Sir Guillum Scott being Chairman of the Society. They presented a *précis* of evidence which a meeting of the Council had decided that they should give on behalf of the Council and the Society. This *précis* was as follows :—

“The Royal Society for the Prevention of Cruelty to Animals was founded to protect animals generally, and not for the suppression of any kind of cruelty in particular. The members of the Society are drawn from all classes, and there is a considerable difference of opinion amongst some of them on various subjects, although on others they are practically agreed. On the question of vivisection, for instance, there is a wide diversity of opinion, many persons objecting to any kind of vivisection, while others object only to painful experiments; others again consider that vivisection is necessary for the advancement of science in the interests of mankind. The attitude taken up by the Society, almost since its foundation, has been that it deprecates all experiments on animals which cause pain, but as regards experiments which cause no pain there is no ground for interference by the Society, because the question of cruelty does not arise. The Society, therefore, is of the opinion that all severely painful experiments should be carried out while the animal is completely under the influence of an anæsthetic, and that it should be destroyed before the effect of the anæsthetic has been removed. The Society has, ever since the passing of the Cruelty to Animals Act, 1876, repeatedly pointed out that the supervision of experiments carried on under licences granted under that Act is totally inadequate. It may not be possible that

every experiment should be performed in the presence of an Inspector, but all those should be which cause pain either in their initial stages or afterwards. All severely painful experiments should only be performed in a particular place or places, so as to facilitate inspection; and all painful experiments should be prohibited, except in the presence of an Inspector. The number of experiments has increased since 1876, from between 300 and 400 in that year, to over 46,000 in 1906, and the number of Inspectors (originally two, and now three) is entirely out of proportion to the increase in the number of experiments. The number of Inspectors is absolutely inadequate, if a proper supervision is to be exercised over the licensees. The Inspectors occasionally witness experiments, but very few. The Inspectors' Reports are drawn up from the information culled from the reports made to them by the licensees. It is impossible for the Inspectors to exercise proper supervision while experiments are going on every day in all parts of the country. The Society is strongly of opinion that additional Inspectors, who shall not necessarily consist of medical men, should be appointed, and that such Inspectors should visit all licensed places frequently, at various times, unknown to the licensees; and that all holders of licences should be required to declare that no experiments, except those duly reported in accordance with the terms of the licence, have been performed; and the licensees should be required to make a return of all animals acquired by them, and how obtained. With a view of securing and maintaining the confidence of the public, the necessity of obtaining the consent of the Home Secretary for the prosecution of a licensee for an alleged offence should be abolished, and it should be left to the discretion of the magistrates, who are empowered to refuse a process if they consider an application is vexatious. The period for taking proceedings should be extended, so

as to be within six months of the publication or the giving information of that which is impugned. The Society, soon after the Act of 1876 came into force, successfully instituted a prosecution under Section 6. The Society has not been interested in, or promoted any other prosecutions."

Under examination, Sir G. Scott stated that the one and only prosecution ever instituted by the Society was against a person called Abrath, who had issued in August, 1876, a large placard, headed "The Balham Mystery," announcing his intention of giving a lecture on antimony, at which he would do experiments on animals, to show the effects of poisons, and to demonstrate his theory that Mr. Bravo was not killed by antimony. It appears from Sir G. Scott's evidence, that Abrath was merely advertising a claptrap lecture on a sensational murder case. He gave his lecture, and raised a laugh against the new Act, which was only a few days old. He did not make any experiments at the lecture. He was had up before the magistrates for publishing an illegal advertisement, and was fined. He held no licence to make experiments on animals; his offence was merely advertising that he would make an experiment in public, which is illegal under the Act. He was convicted simply on the ground of illegal advertising.

SIR GEORGE KEKEWICH, K.C.B., M.P., *December 18, 1907*

Sir George Kekewich, Hon. Secretary of the Parliamentary Association for the Abolition of Vivisection, came to represent that Society. He said that he had no medical knowledge. He thought that any man of ordinary powers of observation and common sense could judge what results had been obtained from experiments on animals. He proceeded to speak of the creation and administration of the Act. He thought that it was obvious that the present

state of medical knowledge was barren. Asked about anæsthetics, he answered, "We have no absolute proof that anæsthetics are ever used, because no anti-vivisection doctor has ever been allowed to witness any experiment." Questioned about plague, he was of opinion that the use of the preventive treatment had been an actual means of propagating the plague. He had never been in India, and had never seen the plague. He was speaking from information gathered from books and conversations, and not from personal knowledge. He had not read the reports of the Plague Commission. He had not read the evidence of the Chairman of the Plague Commission. Asked whether he knew the results of the preventive treatment at the Byculla Jail, he said "No." Asked whether he was aware that the Plague Commission, who were mingling with plague night and day, were all inoculated themselves, and all escaped, he answered that he did not know that. Asked whether he had considered how far plague is infectious or contagious, or not, he said, "What between rats and fleas, and all the rest of it, one does not know any more than one does with many other diseases."

This witness also alluded to the sale of antitoxins. He said that the inventors of sera and antitoxins received royalties upon the sales. He said he would furnish evidence that some British inventor did that.¹ Asked certain questions in ethics, he said that he thought that the medical student was a good deal rougher than he used to be. That was the general view, he should think. He had heard from certain doctors, "whom you would call anti-vivisection doctors," that clinical research was to a great extent subordinated in these days to experimental research. He had not any knowledge of the curriculum required of a medical student. His Association did not consider that any benefit

¹ It appears that this evidence was not furnished.

whatever had been derived from the use of any sera in the treatment of disease. Asked whether his Association had made any inquiries into the conditions of cancer research in this country, he could not say. Asked what medical experts had helped to draw up his *précis*, he named Miss Kenealy and Miss Bouchier. He said that he had very great doubts whether man had the right to kill one of the lower animals. Asked whether he had read Mr. Hobday's evidence before the Commission, he answered, "No, I just looked at it." Asked about vaccination, he said that he objected to it altogether. Asked about diphtheria antitoxin, he said that he had never seen any case where it had been used. Asked whether he could give any instance of any man who had become callous or inhumane after having performed experiments on living animals, under anæsthetics, he answered, "Of course I cannot."

He was asked whether his Association, or he personally, during the whole of the past year, had made any attempt whatsoever to accept Dr. Starling's invitation to visit his laboratory and see the work there for themselves. And he answered: "No, because we should be of no use. . . . What I should like to do would be to be able to see through a hole in the door without it being known that I was there. . . . If we could get admission for Miss Lind-af-Hageby, I dare say it would be very useful to us."



XI

EVIDENCE OF LORD RAYLEIGH AND LORD JUSTICE FLETCHER MOULTON

LORD RAYLEIGH, O.M., P.R.S., *March 5, 1907*

LORD RAYLEIGH, President of the Royal Society, presented a statement on behalf of the Royal Society as follows :

“The Royal Society, from its age and the position accorded to it among scientific institutions, feels its responsibility as a guardian of the general interests of science in this country. Founded as it was for the promotion of natural knowledge, whenever from time to time legislative changes have been proposed which might seem likely to affect the advancement of that knowledge, the Society has desired to make its voice heard on behalf of scientific progress. The recent appointment of a Royal Commission on the subject of experiments on animals has been deemed by the President and Council of the Royal Society to be an occasion when they may ask to be allowed to lay before that Commission a statement of their views on the broad scientific bearings of the question. There can be no doubt that the main cause of the remarkable development of science in modern times has been the adoption of the experimental method of investigating nature. In every department of research this method has led to the most

important advances, both in questions of theory and in practical applications to the useful purposes of life. From the beginning of its history the Royal Society has fostered the prosecution of experiment, and not only in physical and chemical but in biological inquiry, and its publications are full of records of the discoveries which have consequently been made. In no branches of investigation have the theoretical and practical successes of experimental work been more conspicuous in recent years than in physiology and its practical applications in medicine and surgery. In medicine, the careful and patient testing of the effect of drugs on the lower animals has not only led to an accurate knowledge, not otherwise attainable, of these effects as produced on the human body, but has greatly increased the number of substances now available to the physician in the treatment of disease. Without this method of investigation the progress of pharmacology, in recent years so astonishing and beneficent, would be arrested, and diseases, which may in time be successfully combated, would continue their ravages unchecked. In modern surgery the application of similar experimental work has been attended with brilliant success. Most delicate and fundamental operations on the human body have been made possible by the knowledge obtained from the treatment of animals. The President and Council of the Royal Society claim that since the continued advancement of science in every department depends so largely upon the use of the experimental method, the utmost caution should be observed in any proposals for legislation whereby the prosecution of the method might be unduly limited. So much has already been gained from the application of experiments on animals, both for the progress of physiology and for the alleviation of human suffering, and so much more may be confidently expected in the future, that the

President and Council trust that nothing will be done that would hamper the legitimate employment of the method. While precautions should undoubtedly be taken against improper use of experiment on living animals, it is not the province of the Society to suggest what safeguards should be adopted. It is, however, the bounden duty of the President and Council to urge that those safeguards should be so framed as not unnecessarily to interfere with that advancement of knowledge to promote which the Society exists. Such restrictions would not only cripple or arrest the growth in this country of an important branch of biological science, but in so doing would reduce the efficiency of both physician and surgeon to mitigate or cure disease. It might then become no longer possible to maintain the high position which this country has gained in researches necessary for the advancement of knowledge, and for the guidance of medical practice; and the investigators, to whose devotion and skill the progress of medical science owes so much, might be compelled to seek in foreign universities and scientific organisations the opportunities for research which they could no longer find at home. This statement is not founded on general knowledge alone. The co-operation of the Royal Society has often been sought by the Government of this country in taking measures to arrest the spread of deadly disease, and to improve the conditions of health in distant parts of the British Empire. Without the ungrudging services of physiologists and pathologists, many of whom the Society is proud to count among its Fellows, the services thus solicited could not have been given. The President and Council gladly avail themselves of this opportunity of testifying to the laborious and unselfish devotion, often in most dangerous conditions, with which the necessary experimental researches have been carried on, and to the

value of these researches, not only in enlarging our biological conceptions, but in alleviating the sufferings of mankind."

Lord Rayleigh said that this statement had been drawn up, after careful consideration and debate, by the Council of the Royal Society. In reply to questions, he said that the Royal Society, acting in co-operation with the Government, had been concerned with the investigation of Mediterranean fever in Malta, and of sleeping sickness in Africa. He had always heard that the work done was excellent, and he believed that the Royal Society greatly valued the reports of these Commissions, especially the work that had lately been concluded on Malta fever.

LORD JUSTICE FLETCHER MOULTON, F.R.S.,
July 24, 1907

The Rt. Hon. Sir John Fletcher Moulton, Member of the Privy Council, Fellow of the Royal Society, and a Lord Justice of Appeal, said that for many years he had taken a very keen interest in the progress of curative science, both from the scientific side and from the ethical side. In his long experience at the Bar, he had been an interpreter between those who were doing pioneer work in science and those who had to judge of that work, and it was from that standpoint that he now desired to give evidence.¹

Some passages, from Lord Justice Fletcher Moulton's evidence, come well here at the close of all this evidence for and against experiments on animals. They present clearly the cause upheld by the Research Defence Society.

"I think that in the controversy a great deal has been

¹ This evidence has been published in full, for the Research Defence Society, by Macmillan and Co., price one shilling.

lost by looking at it in too narrow a light. It is not a question of vivisection—that is to say, the performing of cutting experiments on living animals; that is a mere nickname, a mere catch-phrase, which was originally invented, I have not the slightest doubt, for purposes of prejudice. I am not saying that with a desire to find fault. One always likes to get a name which expresses one's own point of view without further explanation; but it is a most imperfect representative of the real issue. The real issue is whether curative science is to be an observational science or whether it is to be an experimental science. . . .

“Of old, cruelty was thought nothing of, whether it was to men or animals. You cannot read the history of the world down to, say, the seventeenth century, without feeling that there was an extraordinary callousness to suffering of every kind. A great change from that time has been passing over the world. First, there came the regard for the sufferings of men, and cruelty towards men gradually passed out of favour; and now the thought of human suffering and the desire to avoid it is one of the most unquestioned and influential motives that guide the action of mankind. Presently there followed the extension of the feeling to the sufferings of animals; and there is no doubt that now, with the best part of humanity, sympathy for the sufferings of animals, and the desire to lessen those sufferings, are most influential motives of action. Now that appears to me all in the right direction, and so far as my opinion goes it is not only not contested by either party to this controversy, but both of them would in words accept it as a duty to lessen so far as possible the sufferings of animals as well as of men. I do not wish to be misunderstood when I talk about the sufferings of animals in the same breath, as it were,

with the sufferings of men. I must therefore point out a very broad distinction between the two, which must be borne in mind if we are to come to right conclusions in this matter. The sufferings of animals are substantially physical only. The sufferings of men are not solely physical. Almost as important a factor in that suffering is mental suffering, arising partly from a man's relations to society—that is to say, that death or sickness will cause pain and suffering and trouble and misery to those who are intimately connected with the man—and partly in the individual himself. He is capable of feeling anxiety, regret, dread, and many other things which are of the most serious importance in measuring human suffering, but which are practically absent from animal suffering. I do not say this in any way to detract from what I have said about the importance of avoiding and preventing, so far as we can, animal suffering; it is only that we may keep true our ideas of what suffering is. . . .

“There is an almost unanimous opinion, among those who are engaged in the practical work of the curative sciences, in favour of experimental research in connection with them. That support has been discounted by people saying, ‘Oh, those who are engaged in curative science get callous to pain.’ Now, in my opinion, they are more sensitive to pain, because it is perpetually appealing to them, and, if I might say so, it is the lifelong foe which they are engaged in fighting.

“Those with whom I range myself, who are desirous of extending and supporting experimental research in the curative sciences, consider not only the pain that is inflicted, but they consider the pain that might be prevented, and they hold themselves responsible for permitting pain which they could stop, just as much as for inflicting pain deliberately—they look at the two together. The other school

consider only inflicted pain. They think it their duty to prevent pain being inflicted, even though the infliction of pain may lead to the prevention of many times that much pain in the future.

“I will suppose that a ship which is plague-stricken, and has got rats on board, arrives at a port. A man with a sense of his responsibility, knowing that there is a high probability that rats convey the plague, would unhesitatingly extirpate those rats, even though his only method of doing so was by putting them to a painful death, whether it was by poisoning them with phosphorus, or by stifling them, or by even more painful methods. He would not hesitate; he would feel it his duty to extirpate them. Now, supposing that a person were to come and say, ‘I could not bear to see those poor creatures running for their lives and in danger, trying to save themselves from their relentless pursuers, and so I let as many as I could escape,’ I have no doubt that the person who did that would think that he acted from humane motives. But what would be the consequence? It might communicate plague not only to a town, but to a whole nation. It might bring positively measureless misery. The first man would be right, because he would look at the inflicted pain which would be to the bad side of the ledger. No one would have a right to inflict that suffering merely capriciously. But he would see beyond that pain he was inflicting—that in acting thus he was preventing an amount of pain which was beyond all measure greater than that which he was inflicting; and if he was a man to whom pain appealed, who had a heart which felt keenly suffering whether in men or animals, he would do it all the more unhesitatingly. The other man would think only of the inflicted pain, and say, ‘I am too tender-hearted to inflict it.’ He would not consider that by not

doing it he was causing preventible pain on such an enormous scale. That is typical of the struggle between the two parties. I have chosen that example, not because it is an exaggerated one, or because it is an unfair one. I have chosen it because in that case there is no veil between the act and its consequences. One can see plainly that the letting free of those infected rats might produce those consequences. But now just let me take a hypothesis. I will suppose that, instead of its bringing a plague, the killing of those rats would lead to the discovery of the antiseptic treatment. That antiseptic treatment put an end—substantially, of course, I mean—to an amount of human misery that we can scarcely realise. The suppuration of wounds, accidents leading to months of painful sickness and a recovery which was only partial and left the people maimed, the horrors of war doubled, the deaths in war enormously increased, were all consequences of sepsis—it was just as bad as a plague. Now if, instead of it being a question of ‘Either you extirpate the rats or you have the plague,’ it had been ‘Either you extirpate the rats or the antiseptic method will not be discovered, and the suppuration of wounds and all the horrors which follow from that must remain in the world,’ you see at once that to the thoughtful man the argument in the two cases is precisely the same—‘If I do not inflict this pain, I permit an unmeasured amount of pain which I could prevent.’ In my opinion, when you once see clearly the causal connection between the pain you inflict and the diminution of pain which follows from it, it makes no difference in what way it follows. Your duty is to take that line which produces the minimum total pain, and whether the pain is inflicted pain, or whether it is preventible pain which is not prevented, is in my opinion one and the same thing.

“I think if you were to ask a man who had given thought to the matter, who, of all living men, had been privileged to do most to reduce pain, the answer would, almost without exception, be ‘Lord Lister,’ who introduced the antiseptic method, which has been followed now by the various aseptic and antiseptic methods which obtain all over the world. Yet I saw the other day in a paper that at an anti-vivisection meeting Lord Lister’s name was mentioned, and it was greeted with shouts of ‘Brute.’ They called a ‘brute’ the man who had done most of all living men to lessen pain. And that was, if I might say so, perfectly consistent. They knew that he had voluntarily inflicted pain in his experiments, and that was all that they looked to; and they did not realise that if their real motive, the thing which was the real spring of their action, was the desire to lessen pain, Lord Lister’s name should have been the most highly honoured of all. . . .

“If you look round the world, the only way in which we can diminish pain is by human action. Animals go on regardless of the pain they cause to one another. In fact, the universe is built on pain. Whole races of animals live simply by killing other races, and killing them without the slightest regard to suffering. So that we must look to human action alone to diminish pain. Now, what is it that prevents our diminishing pain in the world? One feels at once that it is not the absence of desire to do it. I believe that desire is very prominently present. It is because we do not know how to do it; it is our ignorance. Take the case of doctors. Doctors are as desirous as they can possibly be of lessening suffering in the world, and yet how often they stand by the bedside, and the patient goes on suffering, and they can do nothing!

“Not only the whole medical profession, but the whole public, agree that doctors ought to be armed with all

the knowledge of the time. There is no difference of view as to this. Every attempt is made to increase the efficiency of the education of the doctor; because we feel that by arming the doctor with the knowledge of the time we arm him as best we can with the means of diminishing pain. That, of course, is true with regard to the knowledge that has been attained during the last, let me take it, forty years—the only years as to which I can speak; and that knowledge has been mainly due to experimental research. Therefore all must feel that this knowledge which has been obtained by experimental research is useful for the purpose of lessening pain, because we try our very best to make all those who are engaged in practical curative work masters of it.

“No man who knows anything of science has any doubt whatever that the right way to advance knowledge is by experiment. You can take the whole range of the sciences, and I would challenge an opponent to name one in which advance, if it has been rapid and striking, has not been through experiment. Where we are reduced to observation, science crawls. Where, and in proportion as, you can use experiment, the science advances rapidly.

“I was puzzling to think of some branch of science where there was only observation, and the only thing I could think of was the case of volcanoes. Now, there is not anything that has attracted human attention more, or fascinated it more, or has been on a bigger scale or more open to observation, than volcanoes; and yet we are almost in the same state of ignorance of the origin and the *modus operandi* of volcanoes as Pliny was. In no way can you bring experiment to bear upon it. As soon as you can bring experiment to bear upon a subject, you are free; but as long as you can merely observe, your progress is very slow. The reason is that experiment is like cross-examina-

tion. You can put the question you want, and Nature always answers it. She does not answer the question you meant to put; she answers the question you actually did put. She swears by the card in the most shocking manner. She does not care in the least what you meant to ask, but she does care what you asked, and she answers it with perfect truthfulness. . . .

"I think that if I were to ask any scientific man of position in the medical profession, 'Where do you look for the solution of the great problem of cancer?' he would say, 'I look to a gradual acquisition of knowledge about it, and the breaking up of the obstinate silence of the disease, which will tell us nothing, neither its preferences nor the things it dislikes, nor its origin, nor anything.' Such men do not give much heed to the investigations aimed directly at its cure, because they do not believe they will succeed. They believe that we shall have to know a great deal more about the disease itself before we know in what direction success can come. But what they do think is this, that by gradually acquiring knowledge of it, knowledge acquired quite independently of whether it is a thing we could use or not, the disease will gradually cease to be a mystery, and when it has ceased to be a mystery we shall find some weak point in its armour. . . .

"I was much amused by some evidence given by one of the lady witnesses who have given evidence before you. She seemed to be very much disgusted with the consequences of all this knowledge, and, if I might paraphrase what she said, it was: 'Nature has made such nice drugs and put them in the plants all around us. Why should we not use these, and not go to such messy things as serum?' Those are not her words, but they express the effect of them. Nature has put all kinds of useful drugs in plants, but she never dreams of using them for curative purposes.

Her method of curing is entirely by means of the messy things with which the researches of the last forty years have led us to work. We are now, thanks to the theoretical knowledge that we have acquired of the cause of these diseases, working to help Nature along the very lines which she has from the foundation of the world herself taken in fighting these diseases.

“You could not have a better example than that which was put before the Commission, namely, the relation of the thyroid gland to myxœdema and cretinism, where, I think, to the surprise of everybody, it was discovered that we have glands within us which are producing protective substances for the normal life, just as Nature will, when there is an attack of a microbic disease, produce *pro hac vice* a protective substance against the foreign invader. When you take away the thyroid gland, and there is not this protective substance, which is no longer formed, then you find the body gets out of order because that is no longer present which was necessary in order to keep down the inherent forces of dissolution. Then it was discovered that if you supplied artificially that which this gland produced, you could keep down this form of disease just in the same way as you can stop the effects of diphtheria, if you put in from another animal that which Nature would produce in order to repel the microbic attack. Just as in diphtheria you get recovery, so you find in myxœdema that when you supply this substance which the body has ceased to have the power of producing, you get this remarkable recovery. And this appears to be only the commencement of a series of analogous successes.

“Just look at the complete change that there is in our knowledge of all those great groups of diseases which I have called infectious or communicable. We are no longer fighting them blindly; we are no longer striking

in the dark; we know what they are. This very knowledge enables us to avail ourselves of experiments which are not painful, for the purpose of assisting us in the task, and every man who feels his responsibility does all he can without occasioning pain. But what is done, is done with a knowledge of the enemy we are fighting, so that the experiments grow more telling. The number of painful experiments, as is shown by looking at the actual experiments which have been made, is quite infinitesimal. I read the description of the experiments done under licence in England, where excellent research work is done, and most carefully done too; I read through the list of those which were thus done in a year, and I very much doubt whether the total amount of suffering caused by those experiments would be much greater than would be caused in a single fairly large shooting party, where there were one or two bad shots. If you consider the amount of suffering that is caused in the world, not only thoughtlessly, but even deliberately done by people who are ordinary normal men, and may be able to show a reasonable defence for what they do, it dwarfs so utterly the amount that is requisite to produce this useful knowledge, that, if the matter was not so serious, it would be almost ludicrous to think that there was this organised opposition to the pain caused in scientific research in proper hands. Why, what is attacked here is the only bit of fruitful pain in the world. The greater part of pain had better not be. A man suffers and dies, or suffers and gets well, and all the pain he has suffered has benefited nobody. And in the case of animals there is all this vast mass of pain which is inflicted or permitted, and people tolerate it and say nothing about it, and look upon it as an ordinary thing; but there is one little bit which brings return in lessening the sufferings of the world, and the people

are to be found to organise themselves against it, and throw the whole of their strength into denouncing and preventing it.

“ If we could get people to learn what science is and what it has done, they would never attack those who are at work on it. And it is to keep them from learning, that there is this frightful misrepresentation. I remember, and I think the Chairman of the Commission probably remembers, how in the 'seventies the walls of London were placarded with a poster representing a rabbit in the process of being roasted alive. The poster was absolutely false. Yet that placard was all over London. It is not surprising that a great number of people join this organisation, because they get their ideas from these very serious misrepresentations. They believe themselves to be humane, but when one realises the evil that ignorance does, and that the only way in which ignorance can be removed is by the experimental method, and the enormous advantages in the way of saving pain that these results have produced, one must feel that the truly humane men are the people who are defending scientific research.”

XII

INSPECTORS' REPORTS FOR 1910

THE following Reports were published in July, 1911. During 1910 there was a marked increase in the number of mice inoculated for Cancer Research: and there was a decrease in the number of dogs or cats used for experiment under Certificate B. These Reports, with the Tables to which they refer, can be obtained, price 6½*d.*, from Wyman & Sons, Fetter Lane, London, E.C., or through any bookseller.

ENGLAND AND SCOTLAND

May 6th, 1911.

SIR,

I have the honour to submit the following Report on Experiments performed in England and Scotland during the Year 1910, under the Act 39 & 40 Vict. c. 77, including—

- (a) The Names of all Persons who have held Licences or Special Certificates during any part of the Year; together with a Statement of the Registered Places at which the Licences were valid, and of the Official Positions of the Persons who signed the Applications for Licences and granted Certificates under the Act.

- (b) The Total Number of Experiments returned during 1910, classified and arranged according to their general Nature.

REPORT

The names of all "registered places" are given in Table I. Nine new places were registered for the performance of experiments, and three places were removed from the register during the year. All licensees were restricted to the registered place or places specified on their licences, with the exception of those who were permitted to perform inoculation experiments in places other than a "registered place," with the object of studying outbreaks of disease occurring in remote districts or under circumstances which render it impracticable to perform the experiment in a "registered place."

The names of all those persons who held licences during 1910 are contained in Tables II. and III. The total number of licensees was 542. Reports have been furnished by (or, in a few exceptional cases, on behalf of) these licensees in the form required by the Secretary of State. The reports show that 147 licensees performed no experiments. The numbers given above include 26 licensees whose licences expired on February 28, 1910, and of whom 24 returned no experiments in 1910.

Tables I., II., and III. afford evidence,—

1. That licences and certificates have been granted and allowed only upon the recommendation of persons of high scientific standing ;
2. That the licensees are persons who, by their training and education, are fitted to undertake experimental work and to profit by it ;
3. That all experimental work has been conducted in suitable places.

Table IV. shows the number and the nature of the experiments returned by each licensee mentioned in Table II., specifying whether these experiments were done under the licence alone or under any special certificate.

Table IV. is divided into two parts, A. and B., for the purpose of separating experiments which were performed without anæsthetics from experiments in which anæsthetics were used.

The total number of experiments included in Table IV. (A.) is 4,939.

Of these there were performed,—

Under Licence alone ¹	2,718
„ Certificate C.	224
„ Certificate B.	1,765
„ Certificate B. + EE.	232

Table IV. (B.) is devoted entirely to inoculations, hypodermic injections, and some few other proceedings,

¹ In experiments performed under licence alone, the animal must during the whole of the experiment be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain; and the animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered.

Certificate C. allows experiments to be performed, under the foregoing provisions as to the use of anæsthetics, in illustration of lectures.

Certificate B. exempts the person performing the experiment from the obligation to cause the animal on which the experiment is performed to be killed before it recovers from the influence of the anæsthetic; and when the animal is a dog or a cat, Certificate EE. is also necessary.

Certificate A. allows experiments to be performed without anæsthetics; and when the animal on which the experiment is performed is a dog or a cat, Certificate E. is also necessary.

Certificate F. is required in all cases of experiments on a horse, ass, or mule.

performed without anæsthetics. It includes 90,792 experiments, whereof there were performed,—

Under Certificate A.	89,963
„ Certificate A. + E.	609
„ Certificate A. + F.	220

The total number of experiments is 95,731, being 9,454 more than in 1909; the number of experiments included in Table IV. (A.) shows an increase of 1,051, and that in Table IV. (B.) an increase of 8,403.

All experiments involving a serious operation are placed in Table IV. (A.). The larger part of the experiments included in this Table, viz., all performed under licence alone and under Certificate C., 2,942 in number, come under the provision of the Act that the animal must be kept under an anæsthetic during the whole of the experiment, and must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic.

In the experiments performed under Certificate B., or B. linked with EE., 1,997 in number, the initial operations are performed under anæsthetics, from the influence of which the animals are allowed to recover. The operations are required to be performed antiseptically, so that the healing of the wounds shall, as far as possible, take place without pain. If the antiseptic precautions fail, and supuration occurs, the animal is required to be killed. It is generally essential for the success of these experiments that the wounds should heal cleanly, and the surrounding parts remain in a healthy condition. After the healing of the wounds the animals are not necessarily, or even generally, in pain, since experiments involving the removal of important organs, including portions of the brain, may

be performed without giving rise to pain after the recovery from the operation; and after the section of a part of the nervous system, the resulting degenerative changes are painless.

In the event of a subsequent operation being necessary in an experiment performed under Certificate B., or B. linked with EE., a condition is attached to the licence requiring all operative procedures to be carried out under anæsthetics of sufficient power to prevent the animal feeling pain; and no observations or stimulations of a character to cause pain are allowed to be made without the animals being anæsthetised.

In no case has a cutting operation more severe than a superficial venesection been allowed to be performed without anæsthetics.

The experiments included in Table IV. (B.), 90,792 in number, are all performed without anæsthetics. They are mostly inoculations, but a few are feeding experiments, or the administration of various substances by the mouth or by inhalation, or the abstraction of blood by puncture or simple venesection. In no instance has a certificate dispensing with the use of anæsthetics been allowed for an experiment involving a serious operation. Inoculations into deep parts, involving a preliminary incision in order to expose the part into which the inoculation is to be made, are required to be performed under anæsthetics, and are therefore placed in Table IV. (A.).

It will be seen that the operative procedures in experiments performed under Certificate A., without anæsthetics, are only such as are attended by no considerable, if appreciable, pain. The certificate is, in fact, not required to cover these proceedings, but to allow of the subsequent course of the experiment. The experiment lasts during the whole time from the administration of the drug, or injec-

tion, until the animal recovers from the effects, if any, or dies, or is killed—a period possibly extending over several days, or even weeks. The substance administered may give rise to poisoning, or set up a condition of disease, either of which may lead to a fatal termination. To administer to an animal such a poison as diphtheria toxin, for example, or to induce such a disease as tuberculosis, although it may not be accompanied by acute suffering, is held to be a proceeding “calculated to give pain,” and therefore experiments of the kind referred to come within the scope of the Act 39 and 40 Vict. c. 77. The Act provides that, unless a special certificate be obtained, the animal must be kept under an anæsthetic during the whole of the experiment; and it is to allow the animal to be kept without an anæsthetic during the time required for the development of the results of the administration that Certificate A. is given and allowed in these cases.

It must not be assumed that the animal is in pain during the whole of this time. In cases of prolonged action of an injected substance, even when ending fatally, the animal is generally apparently well, and takes its food as usual, until a short time before death. The state of illness may last only a few hours, and in some cases is not observed at all.

In a very large number of the experiments included in Table IV. (B.) the results are negative, and the animals suffer no inconvenience whatever from the inoculation. These experiments are therefore entirely painless.

In the event of pain ensuing as the result of an inoculation, a condition attached to the licence requires that the animal shall be killed under anæsthetics as soon as the main result of the experiment has been attained.

The returns show that during the year 1910, 49,662 experiments were performed by 27 licensees, working at

eight institutions, in the course of Cancer investigations. Of these, 816 are in Table IV. (A.) and 48,846 in Table IV. (B.). The latter are almost entirely inoculations into mice.

A large number of experiments, almost wholly simple inoculations and similar proceedings contained in Table IV. (B.), were performed either on behalf of official bodies, with a view to the preservation of the public health, or directly for the diagnosis and treatment of disease. Several County Councils and Municipal Corporations have their own laboratories in which bacteriological investigations are carried on, including the necessary tests on living animals ; and many others have arrangements by which similar observations are made on their behalf in the laboratories of Universities, Colleges, and other Institutions. A sewage farm is registered as a place in which experiments on living animals may be performed, in order that the character of the effluent may be tested by its effects on the health of fish. The Local Government Board and the Board of Agriculture and Fisheries have laboratories which are registered for the performance of experiments having for their object the detection, prevention, and study of diseases of man and animals. In other places experiments have been performed on behalf of the Home Office, the Naval Medical Service, the War Office, the Army Medical Department, the Army Medical Advisory Board, the Army Veterinary Service, the General Post Office, the Local Government Board, the Metropolitan Asylums Board, the Royal Commission on Tuberculosis, the Advisory Committee for Plague in India, the Tropical Diseases and Glass-blowers Cataract Committees of the Royal Society. Eighty licensees return nearly 19,000 experiments which were performed for Government Departments County Councils, Municipal Corporations, or other Public Health Authorities ; and seventeen licensees performed over 8,000

experiments for the preparation and testing of antitoxic sera and vaccines, and for the testing and standardising of drugs.

During the year the usual inspections have been made by Sir James Russell, Mr. J. Ryland Whitaker, M.B., F.R.C.P. (who was appointed temporary Assistant Inspector during the indisposition of Sir James Russell), and myself. We have seen numerous animals under experiment, both of those coming into Table IV. (A.) and of those coming into Table IV. (B.); we have everywhere found the animals suitably lodged and well cared for, and the licensees attentive to the requirements of the Act, as well as to the conditions appended to their licences by the Secretary of State.

The following are the only irregularities that have occurred during the year :—

A licensee holding Certificate A. for the performance of inoculations without anæsthetics inserted materials which were not covered by the terms of his certificate beneath the skin of five rabbits. The Secretary of State was satisfied that this had been done under a misconception of the scope of the certificate, and by his direction the licensee was admonished.

A licensee who held certificates A. F. given for inoculations and injections reported among his experiments some in which he had administered substances to horses by the mouth. The irregularity was pointed out to him, and he was informed that in such cases procedures must not be adopted that are not specified in the certificates.

I have the honour to be,

Sir,

Your obedient Servant,

G. D. THANE,

Inspector.

To the Right Hon. Winston Leonard Spencer-Churchill,
Secretary of State for the Home Department.

IRELAND

29, HATCH STREET, DUBLIN,
March 25, 1911.

SIR,

I have the honour herewith to submit a report of the particulars of the work done in Ireland during the year 1910, under the Act 39 and 40 Vict. c. 77, together with a list of the registered places in Ireland, and such other information as you desire, or as may be proper to the subject.

Table I. shows that there were 15 of such places, and that in two of them no person was licensed to perform experiments during the year 1910.

Twenty-three licences were in force during the year, of which 21 were in existence at the beginning of the year, and two were new licences issued during the year.

The certificates in existence or allowed were :

A.	to 16	licensees.
B.	„ 8	„
C.	„ 6	„
E.	„ 4	„
EE.	„ 4	„
F.	„ 3	„

These include six new certificates, viz. :

Four of A.
Two of C.

Five certificates lapsed, viz. :

Three of A.
Two of C.

The experiments performed were 254 in number, 9 under licence alone, and 245 under certificates. Sixteen licensees

performed experiments, the number of experiments under each certificate being :

Under Certificate A.	213
"	"	C.	.	.	.	30
"	"	EE. (with B.)	.	.	.	2

The animals experimented on were :

Guinea-pigs	135
Rabbits	64
Frogs	13
Mice	12
Pigeons	7
Cattle	5
Dogs	5
Fowl	5
Rats	4
Cats	3

The experiments were of various descriptions, 45 having been physiological, 7 therapeutical, and 202 pathological. The greater number consisted of hypodermic injections and inoculations.

The principal were, investigations of the gases of blood and blood pressure; the movements of respiration, functions of heart and nervous control of heart; intestinal movements in connection with respiration; uterine contraction; nervous and chemical control of digestive glands; the study of X-rays; study of calf vaccine lymph; of medico-legal investigations; of the intravenous injection of drugs in disease; of the study of the tubercle bacillus, and of the pneumococcus; of the pathogenicity of lumbar puncture; of the actions of the kidney; of the action of adrenalin and of an antimeningococcus; experiments on preparation of immune sera; testing virulence of typhoid; effects of section of spinal cord.

Much good work was done in connection with study of

pathology of diseases of the lower animals, for instance, anthrax, fowl cholera, swine fever, swine erysipelas, John's disease, and the pathological organisms in milk and in lard.

The work done was, in my opinion, done sincerely, within the provisions of the Act, and with the best intention.

As I had occasion to mention in my last report, the increase in the experiments of late years is largely connected with the study of new investigations in the matter of the treatment of disease by means of antitoxines, sera, and vaccines, and is therefore of high therapeutic and pathological value, as tending to the development of a new and most important knowledge of disease and its treatment.

I am, etc.,

W. THORNLEY STOKER,

Inspector for Ireland.

To the Right Honourable
the Chief Secretary to the
Lord-Lieutenant of Ireland.

XIII

THE REPORT OF THE ROYAL COMMISSION

THE final Report of the Royal Commission on Vivisection was published on March 12, 1912, four years after the Commission had ceased to hear evidence. It may be procured, price 1s. 3d., from Wyman & Sons, Fetter Lane, E.C., or through any bookseller. Happily, it is unanimous: there is no minority report. Three of the eight Commissioners sign the Report subject to certain reservations, which they put in memoranda. None of the recommendations contained in the Report needs the intervention of Parliament. All such changes as are recommended in it come well within the province of the Home Office and the Treasury.

Both among the Commissioners and among the witnesses the cause of the anti-vivisection societies was very generously represented. The number of anti-vivisection witnesses was eighteen, exclusive of the two representatives of the Royal Society for the Prevention of Cruelty to Animals.

The Report deals with evidence given to the Commission; and the latest of that evidence is now four years old. Thus, the Report is silent over the work done from 1908 onward. Science does not stand still while Royal Commissions deliberate; and the Report has a rather old-fashioned air. It says nothing of Flexner's discovery of

a serum-treatment for epidemic meningitis, nor of his subsequent work on epidemic infantile paralysis; nothing of Ehrlich's recent work on spirochaeta, nor of Ford Robertson's work on general paralysis of the insane; nor does it refer to the admirable results, published by Sir William Leishman in February 1909, of the preventive treatment against typhoid fever among our army in India. Nothing is said of the fact that the Government of the United States has made this treatment compulsory on all officers and men under forty-five years of age in the United States Army—excluding, of course, those who have already suffered from the disease. Nothing is said of the reduction of the death-rate from sleeping sickness in the Uganda Protectorate, from 8,003 in 1905 to 1,546 in 1910. There were contributory causes, but one contribution toward this reduction was the accurate knowledge gained through the experimental study of the disease. Nothing is said of the wiping-out of "Rock-fever" in Gibraltar, by stopping the use of the goats' milk. These and other important illustrations of the value of experiments on animals—for instance, the latest results of the treatment of diphtheria, the latest results of the preventive treatment against rabies, Nuttall's study of malignant jaundice of dogs, and Copeman's study of distemper—did not come before the Commissioners, and are not mentioned in the Report.

The Report begins with a short account (pp. 1 to 10) of previous legislation, and of the text and the administration of the Act. It then deals with certain charges made against the Home Office by Mr. Coleridge (see chap. x. p. 277). The Commissioners do not appear to attach much importance to them. "We are of opinion," they say, "that, on the whole, the working of the Act has been performed with a desire faithfully to carry out the objects which its framers had in view." The Report then refers

to Dr. Crile's experiments (see chap. v. p. 163): and it is to be noted that Colonel Lockwood, one of the Commissioners, speaking in the House of Commons on the day on which the Report was published, stated that the Commissioners, after carefully searching through the whole question, believed that the animals used in these experiments were absolutely senseless and without pain.¹

The Report goes on to certain statements made by Miss Lind-af-Hageby, Mrs. Cook, Lieut.-Colonel Lawrie, and Mr. Graham. "After careful consideration of the above cases, we have come to the conclusion that the witnesses have either misapprehended or inaccurately described the facts of the experiments."

The Commissioners condemn Dr. Pembrey's position. They also make the following statement:

"We desire to state that the harrowing descriptions and illustrations of operations inflicted on animals, which are freely circulated by post, advertisement or otherwise, are in many cases calculated to deceive the public, so far as they suggest that the animals in question were not under an anæsthetic. To represent that animals subjected to experiments in this country are wantonly tortured would, in our opinion, be absolutely false."

The next part of the Report (pp. 21 to 47) is a brief review of the progress of science, and of the results of experiments on animals (see chaps. iii.-viii.). In the course of this review, the Commissioners call attention to the work done, by the experimental study of the diseases of animals, toward saving or safeguarding the lives of animals. They also refer to the many researches made on behalf of the Local Government Board, the County and Urban District Councils, and the Board of Agriculture. Finally, they

¹ "Parliamentary Debates," vol. 35, No. 20, p. 1045.

refer to the public recognition of the value of the experimental method :

“ In recent years, the value of the experimental method has been very largely recognised by the public at large, as well as by various public bodies. This recognition has taken practical shape in various ways, such as :

“(1) The foundation of Schools of Tropical Medicine, subsidised by the Colonial Office and Colonial Governments, and the appointment of research expeditions or commissions to investigate on the spot such diseases as sleeping sickness, plague, malaria, Malta fever, etc.

“(2) The foundation of an Imperial Research Fund for the purpose of investigating cancer.

“(3) The appointment of a Royal Commission to investigate by experimental methods, and otherwise, that great scourge to the human race, tuberculosis.”

At the end of this part of the Report, all the Commissioners come to the safe conclusion, that “ the great preponderance of medical and scientific authority is against the opponents of vivisection. This is more markedly so now than was the case before the Royal Commission of 1875.” They go on to say :

“(1) That certain results, claimed from time to time to have been proved by experiments upon living animals and alleged to have been beneficial in preventing or curing disease, have, on further investigation and experience, been found to be fallacious or useless.

“(2) That, notwithstanding such failures, valuable knowledge has been acquired in regard to physiological processes and the causation of disease, and that useful methods for the prevention, cure, and treatment of certain diseases have resulted from experimental investigations upon living animals.

“(3) That, as far as we can judge, it is highly improbable that, without experiments on animals, mankind would at the present time have been in possession of such knowledge.

“(4) That, in so far as disease has been successfully prevented or its mortality reduced, suffering has been diminished in man and in lower animals.

“(5) That there is ground for believing that similar methods of investigation if pursued in the future will be attended with similar results.”

The next part of the Report (pp. 48-57) deals with the question of the pain involved in experiments on animals, with certain “miscellaneous questions,” and with “the moral question.” Then, after a brief notice of some suggestions offered to the Commission by witnesses, the Commissioners make their recommendations to His Majesty the King:

1. *Inspection* (see chap. i. pp. 15-33). “We are inclined to think, having regard to the present number of licensed premises and experiments, that there would be an adequate increase of inspection if, in place of the existing arrangements, the Chief Inspector were a whole-time officer, and if in addition to him there were three whole-time Inspectors for Great Britain.

“Assuming that it is not thought practicable or desirable to appoint whole-time officers, but that the services of Inspectors in the active practice of their professions should be retained, we think that arrangements should be made to secure a sufficient number of such Inspectors who could give such time to their duties as would be equivalent to the services of the four whole-time men. It is essential that the Inspectors should be qualified medical men of such position as to secure the confidence both of their own profession and of the public.

“As to Ireland, we think that, having regard to the comparatively small number of licensed places and of experiments carried out under the Act in that country, sufficient inspection can be obtained by the services of one or more part-time Inspectors.”

2. *Use of Curare* (see chap. ix. pp. 253-259). “Some of us are of opinion that the use of curare should be altogether prohibited, but we are all agreed that if its

use is to be permitted at all, an Inspector or some person nominated by the Secretary of State should be present from the commencement of the experiment, who should satisfy himself that the animal is throughout the whole experiment and until its death in a state of complete anæsthesia."

3. *Safeguards against Pain after Experiments.* "We recommend :

"(I) That an Inspector should have power to order the painless destruction of any animal which, having been the subject of any experiment, shows signs of obvious suffering or considerable pain, even though the object of the experiment may not have been attained : and

"(II) That in all cases in which in the opinion of the experimenter the animal is suffering severe pain which is likely to endure, it shall be his duty to cause its painless death, even though the object of the experiment has not been attained.

"The above conditions should be attached to certificates. We regret that we cannot recommend any further extension of the 'pain condition.' We are anxious, as far as possible, to prevent or to limit animal suffering in every case. We have recommended that there should be increased inspection, that wide powers should be given to inspectors to order the painless destruction of any animal under experiment, and that in future, although the object of the experiment has not been attained, no animal should be allowed to live in severe pain which is likely to endure. But we do not feel justified in recommending that, when the object of the experiment has not been attained, an experimenter should in all cases be required to destroy the animal immediately it exhibits signs even of severe pain, which might in some cases be only momentary.

"We are satisfied by the evidence that in the great majority of the experiments under the Act the animals do not exhibit any symptoms suggestive of severe pain, and to require the immediate destruction of an animal as soon as it exhibits such symptoms might, in our

opinion, put an insuperable obstacle in the way of investigating many widespread diseases (afflicting both men and domesticated animals) with respect to which further knowledge as to their nature and treatment is in the interest of humanity urgently required.

"It must not be forgotten that it is in the case of diseases which are naturally painful when they attack men or animals that experiments are most likely to involve pain to animals which are experimentally infected; as examples we may instance cancer, cholera, plague, tetanus, rabies, and snake-bite.

"We are compelled to accept the weighty evidence given before us to the effect that the study of animals experimentally infected with some of these diseases has given us knowledge which has been instrumental in saving much mortality and suffering both in man and animals, and we believe that discoveries already made in this way justify the hope that by the same methods knowledge may yet be extended regarding the means of preventing or curing other most painful diseases which are at present scarcely or not at all amenable to treatment. And finally we feel that as long as public opinion sanctions the infliction on animals of pain, which is not only severe but of long duration, in the pursuit of sport, and in carrying out such operations as castration and spaying,¹ or in the destruction of rabbits and of rats and other vermin by traps and painful poisons, it would be inconsistent and unreasonable to go further than we have already gone in limiting experiments which are designed to result and, according to experience, will probably result in preventing or alleviating great human or animal suffering."

¹ "In the months of spring and early summer in this country, farm-places simply, so to speak, seethe with vivisection. Male and female animals have these sensitive organs cut out of their bodies in full consciousness; and this is done on millions of animals annually. We know to a million or two; but there are many millions. You must not think I am exaggerating about it: you will find it from the statistics returned by the Board of Agriculture every year." (Sir John McFadycan, Evidence before the Commission, vol. ii. p. 74).

4. *An Advisory Body.* "We think that the practice followed by various Home Secretaries for nearly thirty years, of obtaining professional advice as a guide in the exercise of their powers and discharge of their responsibility, is a reasonable and proper practice: but in our opinion the recommendations of the Commission of 1875 should be strictly followed.

"These advisers should, as regards Great Britain, be selected by the Secretary of State from a list of names submitted to him by the Royal Society and the Royal Colleges of Physicians and Surgeons in London. No person so selected should be the holder of a licence, and the names of all persons so selected, as well as the names of the scientific authorities under the Act,¹ should be published. The adoption of this suggestion would involve a discontinuance of the present practice of reference to the Association for the Advancement of Medicine by Research.²

"With regard to Ireland, where heretofore there has been no Advisory Body, we think that the Chief Secretary should be advised by a body chosen upon analogous lines."

These are the four chief recommendations of the Report. Two minor recommendations are made: (1) Stricter provisions as to the definition and practice of "pithing." (2) Special records by experimenters in certain cases.

We now come to the memoranda which were mentioned at the beginning of this chapter. The first memorandum recommends that the present distinction, between the *granting* of licences by the Home Office, and the *allowing*, by the Home Office, of certificates *granted* by the scientific authorities, should be abolished; and that the Home Office alone should *grant* licences and certificates. It also recommends the insertion in the Act of a requirement upon all experimenters in every case in which obvious suffering (real or obvious

¹ See chap. i. p. 6: section 11 of the Act. ² See chap. i. p. 17.

suffering) has supervened forthwith painlessly to destroy the animal. It also suggests that the qualifications for the Inspectorate should be wider; and that the special or immediate reporting of experiments should be the rule, not the exception. This memorandum is signed by three of the Commissioners. There is another memorandum, signed by one alone of these three Commissioners: it does not add any recommendations to those already made in the Report. A short appendix to the Report has been published, giving particulars of the procedure in other countries with regard to experiments on animals, and further statements by some of the witnesses.

It remains to note the negative conclusions of the Report: (1) No alteration of the Act is recommended as to experiments at lectures. (2) It is not recommended that experiments should be permitted "for the purpose of attaining manual skill which might hereafter be useful in operations on human creatures."¹ (3) The production of sera and vaccines for commercial purposes ought not to be brought within the provisions of the Act.² (4) It is not

¹ See the text of the Act, chap. i. p. 3; Prof. Starling's evidence, p. 42; Sir Henry Swanzy's, p. 142; and Sir Victor Horsley's, p. 149. Take the case of a young house-surgeon, or a young doctor in country practice, suddenly called up, at night, to perform, for the first time, tracheotomy. It is hardly open to doubt that he would do the operation better on a child if he had already done it on a dog; and, of course, the dog would be anæsthetised, and would be killed under the anæsthetic. A case of this kind is on a different footing from cases where the animal would be allowed to come round from the anæsthetic. But, in these cases also, it is hardly open to doubt that the learning of certain details of operative surgery on anæsthetised animals would help to ensure the saving of human lives.

² The three Commissioners dissent from this conclusion, and recommend that the production of sera and vaccines should be brought under the Act, so far as living animals are used for such purposes.

recommended that cold-blooded animals should be exempted from the operation of the Act. (5) No recommendation is made, but a suggestion is put forward, as to experiments on dogs, cats, and anthropoid apes :

“In view of the variety of practice and the divergence of opinion as to the necessity of employing dogs for experimentation and demonstration, we find some difficulty in deciding upon this important question. Some of us regard the provisions of the existing law as sufficient, some of us would prefer that in the case both of experimentation and demonstration the further special protection given to horses, asses, and mules should be extended to dogs, while some of us would exclude the use of dogs altogether. But if any alteration is made in the existing procedure, the majority of us would agree that the special enactments now applicable to horses, asses, and mules might be extended to dogs, and also to cats and anthropoid apes.”¹

The publication of this Report came at a time when the nation had graver affairs to think of than experimental physiology and pathology. Doubtless, the public will be content to leave the business of the Home Office to the authorities at the Home Office, and to leave the men of science alone. But it is too much to hope that the opponents of all experiments on animals will exercise this forbearance ; and there will be the more need for the work of the Research Defence Society.

¹ For any experiment on a horse, ass, or mule, the licensee must hold, in addition to his licence, Certificate F ; even in the case of inoculations, and in those cases where the animal is under anæsthesia throughout the experiment and is killed without recovering consciousness. An experiment on a dog, cat, or ape, in which the animal is under anæsthesia throughout the experiment and is killed without recovering consciousness, may be made under a licence alone, without a special certificate (see note on p. 318). Of course, if the experiment be made at a lecture, Certificate C is necessary.

Some defects in the Report are : (1) It pays no attention to the advice of Prof. Schäfer, Prof. Gotch, and Prof. Langley (see chap. iii. pp. 54, 58, 69) that animals, allowed to be kept for observation after operation, should be kept, so far as possible, in country air, not in laboratories. (2) It does not recommend that the young doctor should be allowed to learn tracheotomy on a dog under anæsthesia ; he must learn it, really, on children. (3) It does not emphasize the difference of quality between the anti-vivisection evidence and the evidence of men like Lord Rayleigh and Sir Richard Douglas Powell. (4) The added restriction as to curare is likely to increase the present circulation of untruthful statements as to the use of that very rare drug. (5) The change recommended in the " pain condition " seems hardly applicable to those physiologists who have told the Commission that they already observe the proposed rule (see chap. iii. pp. 46, 52, 70). (6) The Report is four years behind the times : it says nothing of many discoveries which are of the highest importance.

Against these defects the Report has this merit, that it is unanimous. It acknowledges valuable work done in the past ; and it looks forward with proper confidence to the work that is waiting to be done by our men of science,

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